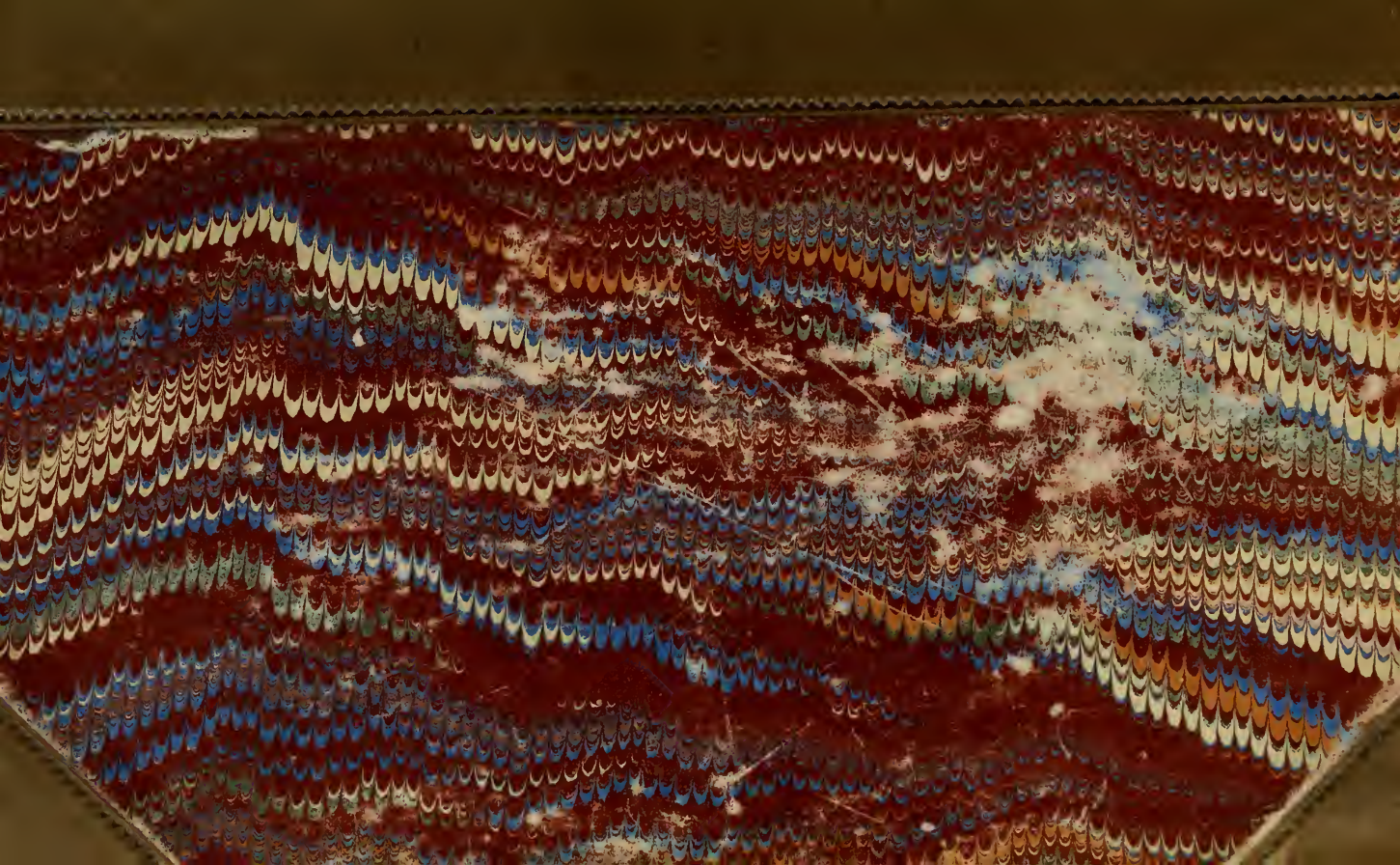

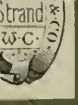
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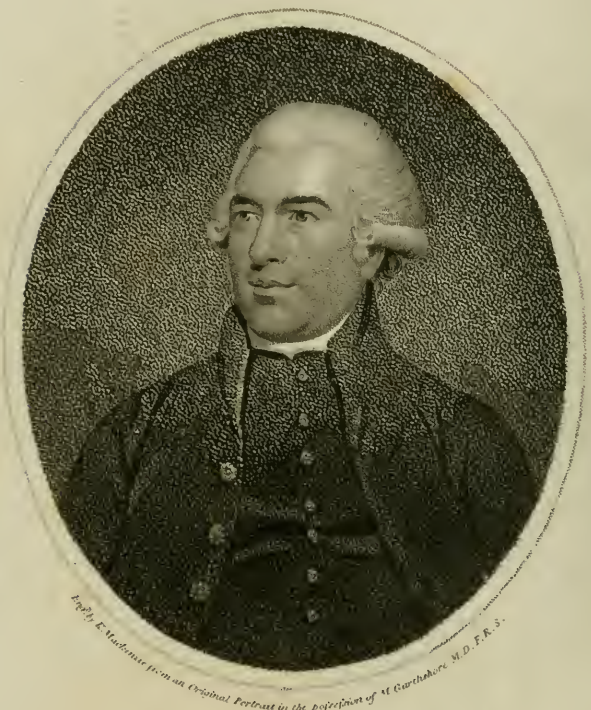

The Right Hon.ble Sir Joseph Banks Bar.t P R S

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, ETC. ETC.

"Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

PHILOSOPHICAL MAGAZINE.

OCTOBER 1800.

I. *Thoughts on Colouring, and particularly with a Retrospect to the Method used by the Venetians in the Mechanical Part of the Art, and to their Method of Arranging the Tints.*

A theory founded on experiment, and not assumed, is always good for so much as it explains. BURKE.

SIR,

SINCE I troubled you with a paper on the subject of Venetian colouring *, I have paid particular attention to such pictures of that school as have occasionally come under my notice, and, in my inquiry, I have been much aided by those in the Orleans collection; but, from all the observations I have been able to make, I cannot help concluding that they never thought of the absorbent ground, on which I made some remarks in my last, and that the dark red ground never formed a part of the picture. Those conclusions I feel authorised in making from the general tendency in the absorbent grounds to produce a hardness, a thing never occurring in the pictures of the Venetians; and that the ground never entered into part of their pictures is evident from their being painted solid throughout, and *the shadows glazed on a body of colour*. I shall, with a view further to illustrate the subject, introduce (occasionally as quotations) such remarks on colouring as I made in my catalogue immediately on my viewing the pictures in the collection above alluded to.

* See Vol. IV.

On looking into the Venetian pictures (particularly thoſe of Titian and Paul Veroneſe), I found that they had all been prepared with colder colours than the finiſhing ones, and that there was one general colour which prevailed throughout the ſhadows; this obſervation induced me to make the following remark: "the Venetians appear to have prepared their pictures in the fleſh, and its ſhadow and draperies, in a colder ſtate, and glazed or ſcumbled them with warmer colours."

One of the greateſt conſiderations with the artiſt is to acquire the true tone of the ſhadow and its dinſy ſhadow; to do this he muſt uſe ſuch colours as will moſt readily ſympathiſe with each other in mixing: to this end it will be found that ivory black, Indian red, and white, will form the beſt general ſhadow colour, which may in the fleſh be improved with vermilion in the reflections. The ivory black, and white, improved with lake, produces a moſt beautiful pearly demi-tint for delicate fleſh, and at the ſame time one of the trueſt and moſt clean that can be acquired. Ivory black with Indian red, or lake, glazes finely, and may be highly improved, when dry, with brown pink, or aſphaltum, uſed with drying oil. This is very like the method I formerly mentioned *, as practiſed by that great man Sir Joſhua Reynolds, to acquire his ſhadows, and whoſe method of colouring was truly Venetian, accompanied with a *chiaro ſcuro* they were ſtrangers to, and a taſte in his portraits unexampled. Thoſe who chooſe to try, will find their advantage in making the ſhadow colour of ivory black, vermilion, and white (as mentioned in Vol. IV.), which will work cleaner than any other combination; and which colours, with the addition of burnt ochre in dark complexions, and yellow ochre in the lights, will be quite ſufficient for all the purpoſes in fleſh for firſt and ſecond colouring, or till the drawing is fixed; the whole being afterwards finiſhed by glazing, to give depth to the ſhadows, and enrich and improve the colouring. Tintoret appears to have forwarded his pictures to nearly their full effect, with a pearly colour of black, red, and white, and to have acquired all his tints by glazing with a pure tranſparent colour, invariably leaving the lights white

* See Vol. IV.

to

to give them brilliancy: Titian, on the contrary, *dead-coloured as near nature as poſſible*, only enriching his colours by glazing, and keeping the ſhadows as near of a tone as poſſible. The abſurdity of uſing dark grounds muſt be obvious to every one who has had the leaſt practice, from the tendency in the white, and all the lighter tints, to ſink into the ground; which accounts for the darkneſs of many of the old pictures, and more eſpecially thoſe that have been painted thin. However deſirable it may be to have one's pictures well coloured, its preſence will by no means compenſate for the loſs of dignity of compoſition, accuracy of drawing or breadths: he that is content to pleaſe by the illuſion of colour, or that flatters the eye by an induſtrious diſplay of tints, muſt not expect to rank high in the art.

> " Mind, mind alone, (bear witneſs Earth and Heav'n!)
> " The living fountains in itſelf contains
> " Of beauteous and ſublime: here hand in hand
> " Sit paramount the Graces." AKENSIDE.

I have often been led to think that the exquiſite clearneſs obſervable in ſome of the pictures of Rubens, ariſes at this time (in a great degree) from his having painted them on white, or, at any rate, light grounds; the advantage of which will be obvious to any one that chooſes to try the following experiment: that is, to ſpread any light colour (as a fleſh colour for inſtance) on a ground prepared half white and half dark; in the courſe of two or three days, that on the white ground will appear clear, while the latter will have ſunk many degrees into the under dark colour. Another advantage attending a light ground is, that the picture is not ſo liable to be injured by cleaning, not to mention its wearing better: two of the fineſt pictures, by Titian, in the Orleans collection (Diana and Acteon, and Diana and Caliſto), bear ſtrong teſtimony to the truth of the above obſervation; the former being in fine preſervation, owing to its having been painted with a great body of colour; while the latter having only a thin coat, the dark red ground appears through the fleſh, to its irreparable injury. This evil would in a great meaſure have been avoided, had the ground been light;

light; and it is evident that the ground was not intended to make the leaſt part of the effect in the picture originally, as the thinneſs in the colour above alluded to, looks as if owing to accident, and the haſte of a man who could ſell all the pictures he painted.

I have already mentioned that the beſt general ſhadow colour is ivory black, &c. which, if the lights and middle tints are coloured as near nature as poſſible, may be enriched by glazing; or the glazing may be done on the middle tints, and the lights of one general hue laid on them, which appears at times to have been the practice of Titian. The following remark on the picture of Diana and Caliſto, may ſerve as a further illuſtration:—"There appears to be yellow in all the lights, even the blue; I therefore infer, that they conſidered the light as giving a colour, or of changing, in a ſmall degree, the local colour of the objects, and that their principle of colouring was thus: *the lights on the objects partook of the colour of the light, the middle tints gave the local colour, and the ſhadows warm, and pretty much of one hue.*" A picture painted on the above plan, will have a harmonious and pleaſant effect.

I wiſh the artiſts of any eminence would communicate the reſult of their practice; much good would ariſe from it. Fine writing is not a neceſſary requiſite; for, in matters intended to inſtruct, clearneſs will more than compenſate for want of elegance: ſuch a conduct would contribute to remove many of the practical difficulties the ſtudent labours under; an inconvenience I myſelf have greatly felt.

It is very rarely that you ſee a diſtinct or whole colour in the Venetian pictures; as their blues ſeldom go beyond a violet, and the reds, a crimſon. "In all the pictures in the room (Orleans collection) by this maſter (Titian), no one deciſive colour is to be ſeen, as they are all broken to correſpond with each other: that is, the red are broken with yellow, yellows with red, and ſo on. He appears to have been particularly careful not to introduce blue or cold colours, and, when they do occur, it is only in ſmall quantities."

I would by no means have it underſtood that I wiſh to tie the artiſt down to a particular manner of colouring; one

of our profeſſional evils is looking too much to art for its rules; but I believe a great ſource of the pleaſure derived from the ſight of the Venetian pictures, is their comfortable warm maſſes: great caution however will be required to diſtinguiſh between a glowing and a glaring colour; as we are apt to admire what is ſhowy, before we can diſtinguiſh what is beautiful. I have frequently had occaſion to remark the ſatisfaction expreſſed by the ſpectators on viewing a warmly coloured picture, and the degree of comfort it has appeared to impart: this has frequently led me to reflect on the application of colour to medical purpoſes, as in the ague or fever, or how much the mind might be tranquiliſed by the room being of a particular colour; every one is ſenſible of the effect of that beautiful amber-coloured light that at times prevails on our ſummer's evenings, and which is ſo highly congenial to health and life *.

In all caſes the good ſenſe of the artiſt muſt be uſed, for the colouring that ſuits one ſubject will not agree with another; moſt of the Venetian pictures that I have ſeen, have been of the pleaſing or ornamental caſt, which require more decoration of colour than the higher walks of hiſtory, but I have never been able to prevail on myſelf to believe the moſt ſublime ſubject would be hurt by grouping the colours. It would be worth inquiring how far the painter might be benefited (as the poet certainly is) by conſidering colours as indicative of the paſſions, and applying them accordingly: the fiery reds convey the idea of anger, and in this ſenſe are often uſed:

> " Should intermitted vengeance arm again
> " His red right hand to plague us? MILTON.

* The fact is it is uſed medically, as in the caſe of weak eyes, but not on ſo extenſive a plan as it might be; for inſtance, if the walls of the chambers where inſane people are confined were to be plaſtered to an uniform ſurface, and coloured of a ſtill green, as near the tranquil colour of graſs as poſſible, it is probable it would contribute to compoſe the ſpirits; inſtead of which the ſight is diſtracted, and in all probability the mind ſet to work in decyphering the fiſſures and irregularities in their ſurrounding old walls, which may ultimately end in provoking that propenſity towards drawing expreſſed by many unfortunate people.

Addiſon

Addiſon, in his Cato, heightens his figure by "red with uncommon wrath." Gray and the ſoft green convey the idea of tranquillity; and the dark blue gray, and brown, of ſorrow and melancholy:

"——————— me, goddeſs, bring,
"To arched walks of twilight groves,
"And ſhadows brown, that Sylvan loves,
"Of pine ———. *Milton's Il Penſeroſo.*

Mellow tones ever aſſociate with gentle, and ſharp ones with violent, emotions.

In the practice of glazing, the leſs oil is uſed the better; if the part is rubbed out, and wiped dry with a ſilk rag (an old ſilk handkerchief is good, but a piece of waſh leather does beſt), then the colour ſhould be driven as much as poſſible and often repeated if neceſſary, except it ſhould be a colour that requires drying oil: and, I believe, on moſt occaſions, if the part is hard, drying oil and turpentine, in equal parts, will be found as good a vehicle as any. In what the artiſt terms *ſcumbling*, or uſing an opaque colour thin, it ſhould be driven with a ſtiff pencil, and as little oil as poſſible.

As a concluſion to the preſent paper, I ſhall add the ſum of my remarks on the Venetian pictures in the Orleans collection:—"From the pictures of Titian and Paul Veroneſe I judge they were particularly careful to group the warm colours; to avoid the uſe of blue and cold ones; *never to ſuffer them to interrupt the principal maſs of light, which with them is always compoſed of warm colours*; that the cold colours were ſparingly introduced for the purpoſe of ſupporting the blue in the ſky or diſtance, as in No. 123, 240, 249*, *and never on the principal object*. In many of their pictures there is a total excluſion of the cold colours, as in 169 and 181; in others, where the colours of the objects are all warm, the ſkies are broken either with warm purple clouds, or the blue of the element reduced to gray, as in 49, 211,

* 123, Holy family in a landſcape, Titian; 240, Diana and Acteon, Titian; and 249, Diana and Caliſto, Titian; 169, an Allegory; and 181, a ditto, Paul Veroneſe.

220 *; their shadows prepared with a gray or pearly colour, enriched with glazing: their draperies glazed; flesh glazed, and scumbled with rich virgin tints; and the whole harmonised by the high lights being touched on with a warm yellow colour, nearly of the tone of the high lights of the flesh, as in remark on No. 249 †." I cannot help adding, there were not any pictures in the Orleans collection which (in point of colour) looked so agreeable as the Venetian, owing to their principal mass of colour being invariably warm; wherever the contrary occurred, the effect on the eye was unpleasant; as also when the great mass was disturbed by the intervention of a cold colour.

I remain, Sir, your humble servant,

E. DAYES.

Francis-street,
April 11, 1800.

The Editor of the Philosophical Magazine.

II. *Experiments on the Refrangibility of the invisible Rays of the Sun.* *By* WILLIAM HERSCHEL, *LL.D. F.R.S.* ‡

IN that section of my former paper which treats of radiant heat, it was hinted, though from imperfect experiments, that the range of its refrangibility is probably more extensive than that of the prismatic colours; but, having lately had some favourable sunshine, and obtained a sufficient confirmation of the same, it will be proper to add the following experiments to those which have been given:—

I provided a small stand, with four short legs, and covered it with white paper §. On this I drew five lines, parallel to one end of the stand, at half an inch distance from each other, but so that the first of the lines might only be $\frac{1}{4}$ of an

* 249, Venus rising from the sea, Titian; 211, the education of Cupid, Titian; 220, Europa, Titian.

† Diana and Calisto, Titian.

‡ From the *Transactions of the Royal Society for* 1800.

§ See Plate I. fig. 2.

inch from the edge. Theſe lines I interſected at right angles with three others; the ſecond and third whereof were, reſpectively, at $2\frac{1}{2}$ and at 4 inches from the firſt.

The ſame thermometers that have before been marked No. 1, 2, and 3, mounted upon their ſmall inclined planes, were then placed ſo as to have the centres of the ſhadow of their balls thrown on the interſection of theſe lines. Now, ſetting my little ſtand upon a table, I cauſed the priſmatic ſpectrum to fall with its extreme colour upon the edge of the paper, ſo that none might advance beyond the firſt line. In this arrangement, all the ſpectrum, except the vaniſhing laſt quarter of an inch, which ſerved as a direction, paſſed down by the edge of the ſtand, and could not interfere with the experiments. I had alſo now uſed the precaution of darkening the window in which the priſm was placed, by fixing up a thick dark green curtain, to keep out as much light as convenient.

The thermometers being perfectly ſettled at the temperature of the room, I placed the ſtand ſo that part of the red colour, refracted by the priſm, fell on the edge of the paper, before the thermometer No. 1, and about half way, or $1\frac{1}{4}$ inch, towards the ſecond: it conſequently did not come before that, or the third thermometer, both which were to be my ſtandards. During the experiment, I kept the laſt termination of viſible red carefully to the firſt line, as a limit aſſigned to it, by gently moving the ſtand when required; and found the thermometers, which were all placed on the ſecond line, affected as follows:

No. 1.	No. 2.	No. 3.	
45	45	44	Here the thermometer No. 1 roſe $6\frac{1}{2}$ degrees in 10 minutes, when its centre was placed $\frac{1}{2}$ inch beyond viſible light.
49	45	44	
51	$44\frac{3}{4}$	44	
$50\frac{1}{4}$	$43\frac{3}{4}$	$43\frac{1}{2}$	

In order to have a confirmation of this fact, I cooled the thermometer No. 1, and placed No. 2 in the room of it: I alſo put No. 3 in the place of No. 2, and No. 1 in that of No. 3; and, having expoſed them as before, arranged on the ſecond line, I had the following reſult:

No.

No. 2.	No. 3.	No. 1.	
44	44	45	Here the thermometer No. 2
47	44	45	rose $2\frac{3}{4}$ degrees in 12 mi-
$46\frac{3}{4}$	44	45	nutes: and being, as has
$46\frac{1}{4}$	44	45	been noticed before, much
			more sensible than No. 1,

it came to the temperature of its situation a short time; but I left it exposed longer, on purpose to be perfectly assured of the result. Its showing but $2\frac{3}{4}$ degrees advance, when No. 1 showed $6\frac{1}{2}$, has also been accounted for before.

It being now evident that there was a refraction of rays coming from the sun, which, though not fit for vision, were yet highly invested with a power of occasioning heat, I proceeded to examine its extent as follows:

The thermometers were arranged on the third line instead of the second; and the stand was, as before, immersed up to the first, in the coloured margin of the vanishing red rays. The result was thus:

No. 1.	No. 2.	No. 3.	
46	46	$45\frac{1}{4}$	Here the thermometer No. 1,
50	$46\frac{1}{2}$	46	rose $5\frac{1}{4}$ degrees in 13 mi-
$51\frac{3}{4}$	$46\frac{3}{4}$	$46\frac{1}{2}$	nutes, at 1 inch behind
$52\frac{1}{4}$	47	$46\frac{3}{4}$	the visible light of the red
			rays.

I placed now the thermometers on the fourth line instead of the third; and, proceeding as before, I had the following result:

No. 1.	No. 2.	No. 3.	
$48\frac{1}{4}$	$48\frac{1}{4}$	$47\frac{3}{4}$	Therefore, the thermometer
$51\frac{1}{2}$	$48\frac{3}{8}$	$47\frac{7}{8}$	No. 1, rose $3\frac{1}{8}$ degree in
			10 minutes, at $1\frac{1}{2}$ inch

beyond the visible light of the red rays.

I might now have gone to the fifth line; but so fine a day, with regard to clearness of sky and perfect calmness, was not to be expected often at this time of the year; I therefore hastened to make a trial of the other extreme of the prismatic spectrum. This was attended with some difficulty, as the illumination of the violet rays is so feeble, that a precise termination of it cannot be perceived. However, as well as could be judged, I placed the thermometers one inch beyond the reach of the violet rays, and found the result as follows:

No. 1.	No. 2.	No. 3.	Here the several indications
48	48	47¾	of the thermometers, two
48	48	47¾	of which, No. 1 and 2,
48	47½	47	were used as variable,
48½	47½	47	while the third was kept
48	48	47½	as the standard, were read

off during a time that lasted 12 minutes; but they afford, as may be seen by inspection, no ground for ascribing any of their small changes to other causes than the accidental disturbance which will arise from the motion of the air in a room where some employment is carried on.

I exposed the thermometer now to the line of the very first perceptible violet light, but so that No. 1 and 2 might again be in the illumination, while No. 3 remained a standard. The result proved as follows:

No. 1.	No. 2.	No. 3.	Here the thermometer No. 1
48	48	47¼	rose 1 degree in 15 mi-
48½	48	47¾	nutes; and No. 2 rose
48¾	48½	47¾	½ degree in the same
49	48½	47¾	time.

From these last experiments, I was now sufficiently persuaded, that no rays which might fall beyond the violet, could have any perceptible power either of illuminating or of heating; and that both these powers continued together throughout the prismatic spectrum, and ended where the faintest violet vanishes.

A very material point remained still to be determined, which was, the situation of the maximum of the heating power.

As I knew already that it did not lie on the violet side of the red, I began at the full red colour, and exposed my thermometers, arranged on a line, so as to have the ball of No. 1 in the midst of its rays, while the other two remained at the side, unaffected by them.

No. 1.	No. 2.	No. 3.	Here the thermometer No. 1
48½	48½	48	rose 7 degrees in 10 mi-
55½	48½	48	nutes, by an exposure to
55½	48½	48	the full red coloured rays.

I drew back the stand, till the centre of the ball of No. 1, was just at the vanishing of the red colour, so that half its ball was within, and half without, the visible rays of the sun.

No.

No. 1.	No. 2.	No. 3.	
$48\frac{1}{2}$	$48\frac{1}{2}$	48	Here the thermometer No. 1 rose 8 degrees in 10 minutes.
$55\frac{1}{2}$	$48\frac{1}{2}$	48	
57	49	$48\frac{1}{2}$	

By way of not losing time, in order to connect these last observations the better together, I did not bring back the thermometer No. 1, to the temperature of the room, being already well acquainted with its rate of showing, compared to that of No. 2, but went on to the next experiment, by withdrawing the stand, till the whole ball of No. 1 was completely out of the sun's visible rays, yet so as to bring the termination of the line of the red colour as near the outside of the ball as could be without touching it.

No. 1.	No. 2.	No. 3.	
57	49	$48\frac{1}{2}$	Here the thermometer No. 1, rose, in 10 minutes, another degree higher than in its former situation it could be brought up to;
$58\frac{1}{2}$	$49\frac{3}{4}$	49	
59	$50\frac{1}{4}$	$49\frac{1}{2}$	
59	50	$49\frac{1}{2}$	

and was now 9 degrees above the standard. The ball of this thermometer, as has been noticed, is exactly half an inch in diameter; and its centre, therefore, was $\frac{1}{4}$ inch beyond the visible illumination, to which no part of it was exposed.

It would not have been proper to compare these last observations with those taken at an earlier period this morning, in order to obtain a true maximum, as the sun was now more powerful than it had been at that time; for which reason I caused the line of termination of visible light, now to fall again just $\frac{1}{2}$ inch from the centre of the ball, and had the following result:

No. 1.	No. 2.	No. 3.	
$50\frac{1}{2}$	$50\frac{1}{2}$	50	And here the thermometer No. 1, rose, in 16 minutes, $8\frac{3}{4}$ degrees, when its centre was $\frac{1}{2}$ inch out of the visible rays of the sun.
$57\frac{1}{4}$	50	$49\frac{1}{2}$	
$58\frac{1}{2}$	50	$49\frac{1}{2}$	
$58\frac{3}{4}$	50	$49\frac{1}{2}$	

Now, as before we had a rising of nine degrees, and here $8\frac{3}{4}$, the difference is almost too trifling to suppose that this latter situation of the thermometer was much beyond the maximum of the heating power; while, at the same time, the experiment sufficiently indicates that the place inquired after, need not be looked for at a greater distance.

It will now be eaſy to draw the reſult of theſe obſervations into a very narrow compaſs.

The firſt four experiments prove that there are rays coming from the ſun which are leſs refrangible than any of thoſe that affect the ſight. They are inveſted with a high power of heating bodies, but with none of illuminating objects; and this explains the reaſon why they have hitherto eſcaped unnoticed.

My preſent intention is, not to aſſign the angle of the leaſt refrangibility belonging to theſe rays, for which purpoſe more accurate, repeated, and extended experiments are required. But, at the diſtance of 52 inches from the priſm, there was ſtill a conſiderable heating power exerted by our inviſible rays, one inch and a half beyond the red ones, meaſured upon their projection on a horizontal plane. I have no doubt but that their efficacy may be traced ſtill ſomewhat further.

The fifth and ſixth experiments ſhow that the power of heating is extended to the utmoſt limits of the viſible violet rays, but not beyond them; and that it is gradually impaired as the rays grow more refrangible.

The four laſt experiments prove that the maximum of the heating power is veſted among the inviſible rays, and is probably not leſs than half an inch beyond the laſt viſible ones when projected in the manner before mentioned. The ſame experiments alſo ſhow, that the ſun's inviſible rays, in their leſs refrangible ſtate, and conſiderably beyond the maximum, ſtill exert a heating power fully equal to that of red-coloured light; and that, conſequently, if we may infer the quantity of the efficient from the effect produced, the inviſible rays of the ſun probably far exceed the viſible ones in number.

To conclude, if we call *light*, thoſe rays which illuminate objects, and *radiant heat*, thoſe which heat bodies, it may be inquired, Whether light be eſſentially different from radiant heat? In anſwer to which I would ſuggeſt, that we are not allowed, by the rules of philoſophiſing, to admit two different cauſes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating from

from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet-coloured light, where they are most refracted, and have the least efficacy. We have traced these calorific rays throughout the whole extent of the prismatic spectrum, and found their power increasing, while their refrangibility was lessened, as far as to the confines of red-coloured light. But their diminishing refrangibility, and increasing power, did not stop here; for we have pursued them a considerable way beyond the *prismatic spectrum*, into an invisible state, still exerting their increasing energy, with a decrease of refrangibility, up to the maximum of their power; and have also traced them to that state where, though still less refracted, their energy, on account, we may suppose, of their now failing density, decreased pretty fast; after which, the invisible *thermometrical spectrum*, if I may so call it, soon vanished.

If this be a true account of solar heat, for the support of which I appeal to my experiments, it remains only for us to admit, that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted under the appearance of light and colours; and that the rest, being stopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occasioning a sensation of heat.

Slough, near Windsor,
March 17, 1800.

Explanation of Plate I. fig. 2.

AB, the small stand. 1, 2, 3, the thermometers upon it. CD, the prism at the window. E, the spectrum, thrown upon the table so as to bring the last quarter of an inch of the red colour upon the stand.

III. *Ex-*

III. *Experiments on the ſolar and on the terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame, or different.* *By* WILLIAM HERSCHEL, *LL.D. F.R.S.**

Part I.

THE word *heat*, in its moſt common acceptation, denotes a certain ſenſation, which is well known to every perſon. The cauſe of this ſenſation, to avoid ambiguity, ought to have been diſtinguiſhed by a name different from that which is uſed to point out its effect. Various authors, indeed, who have treated on the ſubject of heat, have occaſionally added certain terms to diſtinguiſh their conceptions, ſuch as, latent, abſolute, ſpecific, ſenſible heat; while others have adopted the new expreſſions of caloric, and the matter of heat. None of theſe deſcriptive appellations, however, would have completely anſwered my purpoſe. I might, as in the preceding papers, have uſed the name radiant heat, which has been introduced by a celebrated author, and which certainly is not very different from the expreſſions I have now adopted; but, by calling the ſubject of my reſearches, the rays that occaſion heat, I cannot be miſunderſtood as meaning that theſe rays themſelves are heat; nor do I in any reſpect engage myſelf to ſhow in what manner they produce heat.

From what has been ſaid, it follows that any objections that may be alleged from the ſuppoſed agency of heat in other circumſtances than in its ſtate of radiance, or heat-making rays, cannot be admitted againſt my experiments. For, notwithſtanding I may be inclined to believe that all phænomena in which heat is concerned, ſuch as the expanſion of bodies, fluidity, congelation, fermentation, friction, &c. as well as heat, in its various ſtates of being latent, ſpecific, abſolute, or ſenſible, may be explained on the principle of heat-making rays, and vibrations occaſioned by them in

* From the *Tranſactions of the Royal Society for* 1800.

the

the parts of bodies; yet this is not intended, at preſent, to be any part of what I ſhall endeavour to eſtabliſh.

I muſt alſo remark that, in uſing the word *rays*, I do not mean to oppoſe, much leſs to countenance, the opinion of thoſe philoſophers who ſtill believe that light itſelf comes to us from the ſun, not by rays, but by the ſuppoſed vibrations of an elaſtic ether, every where diffuſed throughout ſpace; I only claim the ſame privilege for the rays that occaſion heat, which they are willing to allow to thoſe that illuminate objects. For, in what manner ſoever this radiance may be effected, it will be fully proved hereafter that the evidence either for rays, or for vibrations which occaſion heat, ſtands on the ſame foundation on which the radiance of the illuminating principle, light, is built.

In order to enter on our ſubject with ſome regularity, it will be neceſſary to diſtinguiſh heat into ſix different kinds, three whereof are ſolar, and three terreſtrial; but, as the diviſions of terreſtrial heat ſtrictly reſemble thoſe of ſolar, it will not be neceſſary to treat of them ſeparately; our ſubject, therefore, may be reduced to the three following general heads:

We ſhall begin with the heat of luminous bodies in general, ſuch as, in the firſt place, we have it directly from the ſun; and as, in the ſecond, we may obtain it from terreſtrial flames, ſuch as torches, candles, lamps, blue-lights, &c.

Our next diviſion comprehends the heat of coloured radiants. This we obtain, in the firſt place, from the ſun, by ſeparating its rays in a priſm; and, in the ſecond, by having recourſe to culinary fires openly expoſed.

The third diviſion relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This, as I have ſhown, is to be had, in the firſt place, directly from the ſun, by means of a priſm applied to its rays; and, in the ſecond, we may have it from fires incloſed in ſtoves, and from red-hot iron cooled till it can no longer be ſeen in the dark.

Beſides the arrangement in the order of my experiments which would ariſe from this diviſion, we have another ſubject to conſider. For, ſince the chief deſign of this paper is to

give a comparative view of the operations that may be performed on the rays that occaſion heat, and of thoſe which we already know to have been effected on the rays that occaſion light, it will be neceſſary to take a ſhort review of the latter. I ſhall merely ſelect ſuch facts as not only are perfectly well known, but eſpecially ſuch as will anſwer the intention of my comparative view, and arrange them in the following order.

1. Light, both ſolar and terreſtrial, is a ſenſation occaſioned by rays emanating from luminous bodies, which have a power of illuminating objects; and, according to circumſtances, of making them appear of various colours.

2. Theſe rays are ſubject to the laws of reflection.

3. They are likewiſe ſubject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be ſtopped, in certain proportions, when tranſmitted through diaphanous bodies.

6. They are liable to be ſcattered on rough ſurfaces.

7. They have hitherto been ſuppoſed to have a power of heating bodies; but this remains to be examined.

The ſimilar propoſitions relating to heat, which are intended to be proved in this paper, will ſtand as follows:

1. Heat, both ſolar and terreſtrial, is a ſenſation occaſioned by rays emanating from candent ſubſtances, which have a power of heating bodies.

2. Theſe rays are ſubject to the laws of reflection.

3. They are likewiſe ſubject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be ſtopped, in certain proportions, when tranſmitted through diaphanous bodies.

6. They are liable to be ſcattered on rough ſurfaces.

7. They may be ſuppoſed, when in a certain ſtate of energy, to have a power of illuminating objects; but this remains to be examined.

Before I can go on, I have to mention that the number of experiments which will be required to make good all theſe points, exceeds the uſual length of my papers; on which account, I ſhall divide the preſent one into two parts. Proceeding, therefore, now to an inveſtigation of the three firſt

heads that have been propoſed, I reſerve the three next, and a diſcuſſion which will be brought on by the ſeventh article, for the ſecond part.

1ſt Experiment. Reflection of the Heat of the Sun.

I expoſed the thermometer, which in a former paper has been denoted by No. 3, to the eye-end of a ten-feet Newtonian teleſcope, which carried a *Camera eye-piece**, but no eye-glaſs. When, by proper adjuſtment, the focus came to the ball of the thermometer, it roſe from 52 degrees to 110; ſo that rays which came from the ſun, underwent three regular reflections; one on a concave mirror, and the other two on two plain ones. Now theſe rays, whether they were thoſe of light or not, for that our experiment cannot aſcertain, had a power of occaſioning heat, which was manifeſted in raiſing the thermometer 58 degrees.

2d Experiment. Reflection of the Heat of a Candle.

At the diſtance of 29 inches from a candle, I planted a ſmall ſteel mirror, of $3\frac{4}{10}$ inches diameter, and about $2\frac{3}{4}$ inches focal length †. In the ſecondary focus of it, I placed the ball of the thermometer which in my paper has been marked No. 2; and very near it, but out of the reach of reflection, the thermometer No. 3. Having covered the mirror till both were come to the temperature of their ſtations, I began as follows:

	No. 2. In the Focus.	No. 3. Standard.	
0′	54	54	Here, in five minutes, the thermometer No. 2, received $3\frac{1}{4}$ degrees of heat from the candle by reflected rays. I now covered the mirror, but left all the reſt of the apparatus untouched.
1	55	54	
2	56	54	
3	57	54	
4	$57\frac{1}{4}$	54	
5	$57\frac{1}{4}$	54	

	No. 2. In the Focus.	No. 3. Standard.	
0′	$57\frac{1}{4}$	54	Here, in ſix minutes, the thermometer loſt the $3\frac{1}{4}$ degrees of heat again, which it had gained before. I uncovered the mirror once more.
1	$55\frac{1}{2}$	54	
$1\frac{1}{2}$	55	54	
6	54	54	

* See Phil. Tranſ. Vol. LXXII. p 176. † See Plate I. fig. 1.

o′	54	54
1½	56	54
3½	57	54
5	57½	54

And, in five minutes, the 3½ degrees of heat were regained. In conſequence of which we are aſſured that certain rays came from the candle, ſubject to the laws of reflection, which, though they might not be the rays of light, for that our experiment does not determine, were evidently inveſted with a power of heating the thermometer placed in the focus of the mirror.

3d Experiment. Reflection of the Heat that accompanies the Solar priſmatic Colours.

In the ſpectrum of the ſun, given by a priſm, I placed my ſmall ſteel mirror, with a thermometer in its focus *. It was covered by a piece of paſteboard, which, through a proper opening, admitted all the viſible colours to fall on its poliſhed ſurface, but excluded every other ray of heat that might be either on the violet or on the red ſide, beyond the ſpectrum. Then, placing the apparatus ſo as to have the thermometer in the red rays, but keeping the mirror covered up till the thermometer became ſettled, I found it ſtationary at 58°. Uncovering the mirror, I had as follows:

	No. 2.
o′	58
2	93

Here, in two minutes, the thermometer roſe 35 degrees, by reflected heat. I covered the mirror again, and, in a few minutes, the thermometer expoſed to the direct priſmatic red came down to 58° again. And thus the priſmatic colours, if they are not themſelves the heat-making rays, are at leaſt accompanied by ſuch as have a power of occaſioning heat, and are liable to be regularly reflected.

4th Experiment. Reflection of the Heat of a red-hot Poker.

I placed the ſmall ſteel mirror at twelve inches from a red-hot poker, ſet with its heated end upwards, in a perpendicular poſition, and ſo elevated as to throw its rays on the mirror †. The thermometer No. 2, was placed in its ſecondary focus, and had a ſmall paſteboard ſcreen, to guard its ball from the direct heat of the poker.

* See Plate II. fig. 1. † See Plate I. fig. 1.

No.

	No. 2.	
0′	54½	Here, in 1½ minute, the thermometer roſe 38½ degrees by reflected rays; and, when the mirror was covered up, it fell in the next 1½ minute, 28 degrees.
1½	93	
I covered the mirror.		
3′	65	

On which account we cannot but allow that certain rays, whether it be thoſe that ſhine or not, iſſue from an ignited poker, which are ſubject to the regular laws of reflection, and have a power of heating bodies.

[To be continued.]

IV. *Account of ſome intereſting Experiments, performed at the London Philoſophical Society, reſpecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a number of Gems and other refractory Subſtances ſubmitted to its Action; with a Deſcription of the Apparatus employed* *.

IT is well known that no methods with which we are yet acquainted, are capable of exciting a degree of heat which is at all to be compared with that produced by the agents employed in the operations about to be deſcribed. The ſtriking experiment of the deflagration of iron-wire in oxygen gas is frequently accompanied with phænomena, not hitherto attended to as they deſerve to be, which furniſh ſtrong proofs in ſupport of this aſſertion.

A receiver, or bell-glaſs, that will hold about two quarts, with an aperture at the top of about one inch in diameter (ſuch as is now uſually denominated "a deflagrating glaſs"), is charged with oxygen gas, in the uſual manner, on the ſhelf of a pneumatic tub, the aperture being ſtopped with a cork, or, which is better, covered with a piece of ſlate, or other ſuitable ſubſtance, ground flat ſo as to lie cloſely on its edges. If the veſſel be then removed from the ſhelf into an earthen plate, with a ſufficient quantity of water in it to cover the edge of the receiver, and prevent the eſcape of the gas from

* From the unpubliſhed minutes of the Society. We are promiſed, from the ſame quarter, ſeveral other intereſting communications.

below,

below, the phænomena will be the better obſerved. The wire being prepared by coiling it up into a ſpiral, like a cork-ſcrew, inſerting its ſuperior extremity into a cork or cover, adapted to the ſuperior aperture of the receiver, and winding a little cotton round its lower extremity; the cotton is to be very ſlightly dipped in the melted tallow of a candle, and then ſet on fire. This gives ſuch a degree of temperature to the wire as occaſions it to take fire when introduced into the gas, which is done by removing the cork or cover from the receiver and ſubſtituting that containing the wire at the moment of its ignition.

The wire then begins to deflagrate, and little globules of melted iron are formed at its extremity; each falling off in ſucceſſion when it acquires a weight ſufficient to detach it from the remainder of the burning metal. One circumſtance, which cannot fail to impreſs the mind ſtrongly with the intenſity of the heat produced in this experiment, is, that many of theſe globules, though they muſt neceſſarily firſt paſs through the water in the plate, are always found, even when the water is two inches or upwards in depth within the receiver, to have fuſed the vitreous glazing completely through, and to be ſo ſtrongly united with it, that they cannot be afterwards ſeparated without detaching the glazing, together with the ſemi-vitrefied and flag-like globule which adheres to it.

Another effect, which may be produced at pleaſure, furniſhes, if poſſible, a ſtill more convincing proof of the intenſity of the heat. The drops of liquid iron generally acquire the ſize of a ſmall pea before they detach themſelves. If the experimenter, watching the moment, give a quick jerk to the wire, and make the globule ſtrike the ſide of the glaſs, it will often be found to melt its way through in an inſtant, leaving a hole in the glaſs, whoſe edges are as well defined as if it had been drilled. The glaſs uſed on this occaſion in the Society, and which is ſuch a one as Mr. Varley, the experimenter to this inſtitution, makes uſe of in his public lectures, is generally as thick as a ſtrong drinking tumbler; and it is worthy of remark, that, though in charging the glaſs, perhaps not a minute before the experiment,

it

it muſt neceſſarily be immerſed in water, neither its coldneſs or humidity hinders the paſſage of the globule, which ſeems to make its way through with as much eaſe as it would through a piece of writing paper. When the jerk is leſs powerful, the ball of iron will ſometimes not go quite through, but lodge itſelf in the ſubſtance of the glaſs, which it in that caſe generally ſoftens ſo as to raiſe a protuberance on the outſide.

The contemplation of ſuch powerful effects of heat as theſe, induced the Society, on the ſuggeſtion of a very worthy member, and zealous cultivator of the ſciences, Mr. Francillon, to enter upon the preſent experiments, which were performed upon the different ſpecies of gems hereafter enumerated; for all of which the Society was indebted to the politeneſs of the gentleman who firſt propoſed the ſubject, and from whoſe very valuable cabinet they were accordingly ſupplied.

The experiments were made in the preſence of the Society at different meetings, from 24th September to 26th November 1798, in the order in which they are here mentioned; the preſent account being compiled from the original minutes taken by Mr. Turton, their ſecretary, with that degree of accuracy which uſually characteriſes them, and which have again been compared with the gems in their preſent ſtate.

The apparatus employed will be deſcribed at the end of the preſent communication.

Diamonds.

I. The firſt ſtone ſubjected to experiment was a roſe diamond of the weight of $\frac{5}{64}$ths of a carat. It was put upon a piece of compact charcoal in which a ſmall excavation had been previouſly made, and the flame of a lamp, urged by a common blow-pipe, then thrown into the cavity. When the charcoal was ignited it was preſented to another blow-pipe, ſupplied by the gaſometer with an uninterrupted ſtream of oxygen gas.

The diamond, at the end of one minute and fifty ſeconds, was found to weigh only $\frac{4}{64}$ths, having loſt $\frac{1}{13}$ths of its original

ginal weight. It had loſt its tranſparency, figure, and politure.

Bergman obſerves, that the diamond, urged by a very intenſe heat, contracts a ſort of foot upon its ſurface: no appearance of this kind was obſerved on the preſent occaſion.

The unconſumed portion of the diamond, which was recut after the experiment, now exhibits exactly the ſame ſpecies of luſtre as before. It originally "drew colour," as jewellers term it; that is, it had a ſlight brown tinge throughout, ſomewhat like that of a ſmoked topaz, but very pale. It has ſtill the ſame colour. Mr. Francillon, whoſe experience in matters of this nature is equal to that of moſt perſons, made an obſervation on this circumſtance which deſerves to be mentioned. It ſeems that there are ſome diamonds which, although in themſelves colourleſs, contain "ſouls" or "ſpecks," as they are called, which are cavities filled with red, yellow, or brown earths; and theſe colours, being reflected through the tranſparent ſubſtance of the ſtone when cut, give it the appearance of being itſelf coloured. When ſuch ſtones are ſubjected to heat, the earth in theſe cavities turns black; the ſtone after this "plays colourleſs," and the defect is cured. Thoſe which "draw colour," that is, which are themſelves tinged, undergo no change by being heated.

II. Another diamond, which weighed $\frac{1}{64}$ths of a carat, was ſubjected to heat excited by oxygen gas in the ſame manner as the former. At the end of $1' 53''$ it was withdrawn, and was found to have loſt $\frac{1}{64}$ths, or $\frac{1}{17}$ths of its original weight. In a few ſeconds more, it would have entirely diſappeared. It was indeed the intention of the Society to have produced this effect; but one of thoſe fortunate circumſtances which ſometimes lead to conſiderable diſcoveries, preſented on this occaſion a phænomenon even more intereſting than that of the total diſſipation of the gem, though no leſs to have been expected. The diamond was at this time accidentally thrown from the charcoal, and *was clearly and diſtinctly ſeen to flame as a combuſtible body in its paſſage through the air.* The flame was of a blueiſh pearl colour, and nearly

nearly three quarters of an inch in length. This experiment has ſince been frequently repeated, and always ſucceeds in nearly the ſame manner.

This ſtone remains ſtill in the ſtate in which the experiment left it. Its figure and poliſh are quite gone, and it is ſtudded all over with carbonaceous matter of a ſomewhat metallic brilliancy, which, on examination with a magnifier, appears to be plumbago, in union with the ſurface of the diamond.

Amethyſts.

III. An amethyſt, weighing $1\frac{1}{10}$ carat, in 40″ broke into ſeveral pieces. At the end of 1′ 55″ the fragments were withdrawn and again weighed, as there was reaſon to ſuſpect that ſome of them had flown off. Some of them, weighing $\frac{25}{64}$ths of a carat, were then taken and expoſed to the heat for 1′ 15″, when they became fuſed into one maſs, which, on cooling, again broke into ſeveral pieces.

The colour of the ſtone is gone; the fragments are ſemi-tranſparent, and very much reſemble fragments of cryſtals of a pure white ſalt; ſuch, for inſtance, as purified nitre. The fuſed maſs might perhaps have been preſerved entire, if the precaution of cooling it ſlowly, by placing it under a muffle, or in a crucible, in a furnace, and ſuffering the fire to die away gradually, had been taken; but this was not attempted.

IV. Another amethyſt, weighing half a carat, expoſed to the heat for 5′, was fuſed into a globule which is nearly ſpheroidal and pretty ſmooth, but full of ſmall bubbles and cracks, ſimilar to thoſe in glaſs which is thrown, when heated, into water. The whole is colourleſs and tranſparent like flint glaſs, except a portion on one ſide, which, having probably been leaſt heated, looks like white enamel. The ſtone loſt $\frac{1}{32}$ part of its weight.

Sapphires.

V. An oriental ſapphire, weighing $1\frac{5}{64}$ carat, though the heat was applied with caution, by expoſing it firſt to the action of a lamp urged by a common blow-pipe, broke into three pieces, but ſeemed to have undergone no other change

at the end of one minute. The fragments were then ſubjected to the heat excited by a ſtream of oxygen gas thrown upon the charcoal for 2′ 6″. Two of the pieces became colourleſs and have ſome reſemblance to alum; the third retains, in ſome meaſure, its original colour. Their preſent weight is $\frac{+1}{64}$ths of a carat. They have all of them loſt a conſiderable portion of their tranſparency, but have a rough poliſh, and retain their facets.

VI. Another ſapphire, in the form of a parallelopipedon, weighing $\frac{3}{64}$ths of a carat, being expoſed to the ſtrong heat for 2′ 53″, had its angles rounded off, ſo that it became reduced to the form of a tamarind-ſtone, and loſt its tranſparency. It is now a ſemi-tranſparent whitiſh maſs, but ſtill retaining ſome traces of its original colour. It is in part covered and interſperſed with a kind of brown ſcoriæ very like copper bronze. The weight is the ſame as at firſt.

VII. Seven ſapphires, weighing together $\frac{+8}{64}$ths of a carat, heated for 9′ 55″, loſt no weight, but became agglutinated together; the fuſed coat in which they are enveloped having the appearance of a whitiſh ſemi-tranſparent vitreous cement, through which the remainder of the ſtones are ſeen ſtill retaining ſomewhat of their colour. One portion or face of the maſs, containing two of them joined as already mentioned, was afterwards ſubjected to the lapidary's wheel: it took a good poliſh, and proved as hard as at firſt. The appearance of the different parts of the poliſhed face, is the ſame as that of other ſimilar parts of the maſs.

Oriental Topazes.

VIII. A topaz, weighing $\frac{+5}{64}$ths of a carat, was expoſed for 2′ 19″ to a heat excited by oxygen gas, thrown upon the charcoal on which it lay, in two ſtreams, by means of a double blow-pipe of a new conſtruction, ſuggeſted and executed by Mr. Varley, and which will be deſcribed, along with the other parts of the apparatus, in its proper place. The ſtone entirely loſt its colour, and now reſembles a piece of borax. The fuſion, if there was any, was extremely ſuperficial, being only juſt ſufficient to deſtroy the poliſh, the form and facets ſtill remaining, with the angles a little obtunded. Its weight is the ſame as at firſt.

IX.

IX. Three more, weighing $\frac{3}{64}$ths of a carat, heated for 4′ 26″, became agglutinated together, but were found to have loſt none of their weight. Being heated a ſecond time for 4′ 30″, they melted into a globule, which broke in cooling. The principal portion is roundiſh, and the fuſion has evidently been complete: it has a browniſh appearance on the fuſed ſurface, but is clearer in the fracture; the whole being ſomewhat like a piece of dirty camphor. The weight is a little increaſed.

X. A topaz, weighing $\frac{1}{64}$ths of a carat, after being expoſed to the heat for 4′ 33″, was found to have loſt $\frac{3}{64}$ths. It had, like the laſt, loſt its colour, but ſtill partly retains its original figure. One ſmall ſurface was afterwards tried on the lapidary's wheel, and found to be as hard as before.

Brazilian Topaz.

XI. A Brazilian topaz, weighing $\frac{1}{64}$ths of a carat, after being in the heat for 3′ 26″, came out $\frac{2}{64}$ths. It was perfectly fuſed, and had become of an opake white, like a fuſed ſalt; ſuch, for example, as *ſal prunellæ*.

Oriental Rubies.

XII. One weighing $\frac{3}{64}$ths of a carat, heated for 2′ 9″, ſeems ſlightly and ſuperficially fuſed: it is cracked, but the pieces have not ſeparated. The weight and colour are the ſame as before. It is well known that rubies, when expoſed to a common lamp and blow-pipe, become colourleſs, but recover their colour ſuddenly and inſtantly on cooling, when a little below a red heat. This ruby, after its expoſure to the intenſe heat excited by the decompoſition of the oxygen gas, reſumed its colour as it would have done if it had been expoſed to a lower heat.

XIII. Five rubies, weighing together half a carat, were expoſed to the heat for 4′ without loſing any weight. The whole became agglutinated into a ſhapeleſs maſs, in which the remains of the original ſtones now appear interſperſed in a white vitreous cement. The colour of the ſmaller ſtones was changed whitiſh, but one larger portion was but little altered in this reſpect.

XIV. Eight more, weighing $1\frac{8}{64}$ carat, heated for 5′ 8″, loſt no weight: they were agglutinated, like the former, into a ſhapeleſs maſs (which afterwards broke in two), with the ſame appearance of a whitiſh vitreous matter interpoſed. One ſmall portion of the ſurface ſtill retained a fine poliſh. Another part of it was afterwards poliſhed by the lapidary: the ſtone was found to be as hard as before.

XV. To prove whether a longer expoſure would not produce a more perfect fuſion, ſeven more, which weighed together half a carat, were ſubjected to the heat for 9′ 2″, but without undergoing any other change than that of being ſufficiently fuſed to be firmly agglutinated together by a white vitreous cement: the colour of the ſtones themſelves remains, in a certain degree, unaltered.

XVI. Nine rubies, weighing $\frac{48}{64}$ths of a carat, were intended to be expoſed for a ſtill longer portion of time, but, after 6′ 10″, were obſerved to be in fuſion. They were then withdrawn, and the maſs weighed, but no loſs of weight was perceptible. They now compoſe an opake, dirty-looking, whitiſh maſs, ſomewhat like melted camphor, and which has evidently been perfectly fuſed. A few ſmall portions ſtill retain ſomewhat of their original colour; but would probably, alſo, have become white or gray, by a longer continuance of the heat.

It is, perhaps, worthy of obſervation, that a happy accidental arrangement of the ſubſtances employed, will ſometimes produce inſtantaneouſly more powerful effects than a longer expoſure under other circumſtances. This was the caſe in the preſent inſtance; for the weight of the rubies uſed in the laſt experiment, was to that of thoſe employed in the preceding one, as 3 to 2, and yet, though the heat was applied for little more than 2-3ds of the time, the effects were much more ſtriking.

Some remark may perhaps alſo be applicable with regard to the white vitreous ſubſtance into which the ſurfaces of the ſtones become changed. Is it probable that the alkali of the charcoal contributed to deſtroy the colour of the ſtones? or, Would a carbonaceous ſubſtance, containing no alkali, have exhibited with them the ſame phænomena in all reſpects?

This,

This, and other queſtions which will ſuggeſt themſelves, can only be ſatisfactorily anſwered by experiment. It ſeems at leaſt poſſible, however, that the reſults might be different if fuſion could be effected under circumſtances which ſhould preclude the poſſibility of extraneous admixture. It is, perhaps, not altogether abſurd to ſuppoſe that the maſs might in that caſe entirely retain its original colour; and that at ſome future, and perhaps not a very diſtant period, ſmall ſtones and fragments may be fuſed into larger maſſes poſſeſſing all the properties of the natural gems with as much certainty as metals are now caſt into ingots.

[To be continued.]

V. *An Eſſay on the Declivities of Mountains.* *By* RICHARD KIRWAN, *Eſq. LL.D. F.R.S. and Preſident of the Royal Iriſh Academy* *.

AMONG the various cauſes to whoſe activity the planet we inhabit owes its preſent wonderfully diverſified appearance, ſome undoubtedly exerted their influence from its very origin, and others at ſubſequent periods; of theſe laſt, one at leaſt, namely, the Noachian deluge, was univerſal in its operation, while the effects of many more were partial and local, ſuch as thoſe reſulting from earthquakes, volcanos, particular inundations, &c.

In a general ſurvey of the globe it is only to general cauſes whoſe operation was univerſal that our attention can be directed, the effects of partial cauſes being the proper objects of the geological hiſtory of thoſe countries that were particularly affected by them.

But to diſtinguiſh cauſes of the former claſs from thoſe whoſe operation was more confined, it is neceſſary to diſcover ſome character by which their effects may unequivocally be diſcerned.

Now, a general *uniformity*, or agreement in ſome particular circumſtance, in every part of the globe, ſeems to be a ſureteſt of the operation of ſome general cauſe. The diſcovery of uniform appearances is therefore of primary importance

* From the *Tranſactions of the Royal Iriſh Academy for* 1800.

in

in geological researches. In the present essay I shall confine myself to the investigation of one instance of this sort, namely, the *inequality of declivity* which the sides or flanks of mountains exhibit, in every part of the globe hitherto examined, according to the points of the compass to which they face, and are exposed.

That one part of almost every high mountain or hill is steeper than another, could not have escaped the notice of any person who had traversed such mountains; but that Nature, in the formation of such declivities, had any regard to different aspects or points of the compass, seems to have been first remarked by the celebrated Swedish geologist Mr. Tilas, in the 22d Vol. of the Memoires of Stockholm for 1760 *. Neither Varenius, Lulolph, nor Buffon in his Natural History published in 1748, have noticed this remarkable circumstance.

The observation of Tilas, however, relates only to the extreme ends, and not to the flanks of mountains: with respect to the former, he remarked that the *steepest* declivity always faces that part of the country where the land lies lowest, and the *gentlest* that part of the country where the land lies highest; and that, in the southern and eastern parts of Sweden, they consequently face the east and south-east, and in the northern the west. The essential part of this observation extends therefore only to the general elevation or depression of the country, and not to the bearings of these declivities.

The discovery that the different declivities of the flanks of mountains bear an invariable relation to their different aspects, seems to have been first published by Mr. Bergman in his Physical Description of the Earth, of which the second edition appeared in 1773. He there remarked that, in mountains that extend from north to south, the western flank is the *steepest* and the eastern the *gentlest*: and that, in mountains which run east and west, the southern declivity is the steepest, and the northern the gentlest. Vol. II. § 187.

This assertion he grounds on the observations related in his first Vol. § 32, namely, That 1stly, in Scandinavia, in the Suevoberg mountains that run north and south, separating

* See also Vol. XXV. Swed. Abhandl. p. 291, where Cronstedt explains some obscure parts of Tilas's observation.

Sweden

Sweden from Norway, the weſtern or Norwegian ſides are the ſteepeſt, and the eaſtern or Swediſh the moſt moderate; the verticality or ſteepneſs of the former being to that of the latter as 40 or 50 to 4 or 2 *.

2dly, That the Alps are ſteeper on their weſtern and ſouthern ſides than on the eaſtern and northern.

3dly, That in America the Cordelieres are ſteeper on the weſtern ſide, which faces the Pacific Ocean, than on the eaſtern: but he does not notice a few exceptions to this rule in particular caſes, which will hereafter be mentioned.

Buffon, in the firſt Volume of his Epochs of Nature, publiſhed in 1778, p. 185, is the next who notices the general prevalence of this phenomenon, as far as relates to the eaſtern and weſtern ſides of the mountains that extend from north to ſouth, but he is ſilent with reſpect to the north and ſouth ſides of the mountains that run from eaſt to weſt; nay, he does not ſeem to have a juſt comprehenſion of this phenomenon, for he conſiders it conjointly with the general dip of the regions in which theſe mountains exiſt. Thus he tells us, Vol. I. p. 185, that in all continents the general declivity, taking it from the ſummit of mountains, is always more rapid on the weſtern than on the eaſtern ſide; thus the ſummit of the chain of the Cordelieres is much nearer to the weſtern ſhores than to the eaſtern: the chain which divides the whole length of Africa, from the Cape of Good Hope to the Mountains of the Moon, is nearer, he ſays, to the weſtern than to the eaſtern ſeas: of this, however, he muſt have been ignorant, as that tract of country is ſtill unknown.

The mountains which run from Cape Comorin through the peninſula of India, are, he ſays, much nearer to the ſea on the eaſt than on the weſt: he probably meant the contrary, as the fact is evidently ſo; and ſo he ſtates it in the ſecond Volume, p. 295: the ſame, he tells us, may be obſerved in iſlands and peninſulas, and in mountains.

This remarkable circumſtance of mountains was, notwithſtanding, ſo little noticed, that in 1792, the author of an excellent account of the territory of Carliſbad in Bohemia, tells

* The verticality of the ſides is inverſely as the length of the deſcent.

us he had made an obſervation, which he had never met with in any phyſical deſcription of the earth, namely, that the ſouthern declivity of all mountains was much ſteeper than the northern, which he proves by inſtancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian, and Mount Hæmus in Turkey. 2 Bergm. Jour. 1792, p. 385, in the note.

Herman in his Geology, publiſhed in 1797, p. 90, has at leaſt partially mentioned this circumſtance, for he ſays that the eaſtern declivities of all mountains are much gentler and more thickly covered with ſecondary ſtrata, and to a greater height, than the weſtern flanks, which he inſtances in the Swediſh and Norwegian mountains, the Alps, the Caucaſian, the Appennine and the Ouralian mountains; but the declivities, bearing a ſouthern or northern aſpect, he does not mention.

La Metherie, in the fourth Volume of his Theory of the Earth, of which the ſecond edition appeared in 1797, a work which abounds in excellent obſervations), p. 381 *, produces numerous inſtances of the inequality of the eaſtern and weſtern declivities, but ſcarce any of the northern and ſouthern, whoſe difference he does not ſeem to have noticed; but he makes a remark, which I have not ſeen elſewhere, that the coaſts of different countries preſent ſimilar declivities.

With regard to eaſtern and weſtern aſpects, he thinks that a different law has obtained in Africa from that which has been obſerved in other countries; for in that vaſt peninſula he imagines the eaſtern declivities of mountains are the ſteepeſt, and the weſtern the gentleſt. Of this, however, he adduces no other proof but that the greateſt rivers are found on the weſtern ſide: this proof ſeems inſufficient, as, if mountains be ſituated far inland, great rivers may flow indiſcriminately from any ſide of them, and ſometimes few rivers flow even from the ſide whoſe deſcent is moſt moderate, for inſtance, from the eaſtern ſide of the mountains of Syria: the Elbe and the Oder, two of the greateſt rivers in Germany, take their courſe from the weſtern ſides, the firſt of the

* It is to be regretted that he ſcarce ever quotes his authorities.

Bohemian

Bohemian and the other of the Moravian mountains, which yet are the ſteepeſt; many originate from lakes, as the Shannon with us; many take ſuch a winding courſe, that, from a bare knowledge of the place of their diſembogüement, it is impoſſible to judge from what ſide of a mountain they iſſue, if from any; their courſe at moſt diſcovers the depreſſion of the general level of the country.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foſter, publiſhed a geological tract, which merits ſo much more attention as all the facts were either obſerved by himſelf, or related to him by the immediate obſervers. In this he ſtates, as a fact univerſally obſerved, that the ſouth and ſouth-eaſt ſides of almoſt every mountain are ſteep, but that the north and north-weſt ſides are gently covered, and connected, with ſecondary ſtrata in which organic remains abound, which he illuſtrates by various inſtances, ſome of which have been already, and others will preſently be, mentioned.

At preſent this fact attracts the greateſt attention, being obviouſly connected with the original ſtructure of the globe, and clearly proving that mountains are not mere fortuitous eruptions, unconnected with tranſactions on the ſurface of the earth, as has of late been confidently advanced.

I ſhall now ſtate the principal obſervations relative to this object that have been made in different parts of the world.

In Europe.

1° The mountains that ſeparate Sweden from Norway extend from north to ſouth; their weſtern ſides are ſteep, and the eaſtern gentle. 1 Bergm. Erde Beſchreib. p. 157.

2° The Carpathian mountains run from eaſt to weſt; their ſouthern ſides, towards Hungary, are ſteep; their northern, towards Poland, moderate. Foſter, § 46.

3° Doctor Walker, profeſſor of natural hiſtory at Edinburgh, obſerved that the coaſts and hills of Scotland are ſteeper and higher on the weſtern ſide than on the eaſtern. Jamiſon's Mineralogy of Shetland, p. 3.—However, Jamiſon obſerved that the ſouth ſide of the iſle of Arran is the loweſt, and the north ſide the higheſt: p. 51.

4° The mountains of Wales are gentle on the eaſtern and ſteep on the weſtern ſides.

5° The mountains of Parthery, in the county of Mayo, are ſteep on the weſtern ſide.

6° The mountains which ſeparate Saxony from Bohemia deſcend gently on the Saxon or northern ſide, but are ſteep on the Bohemian or ſouthern ſide. Charpent. p. 75.—The ſouthern declivity is to the northern as ſix to two. 2d Bergm. Journ. 1792, p. 384 and 385.

7° The mountains which ſeparate Sileſia from Bohemia run nearly from eaſt to weſt, yet are ſteeper on the northern or Sileſian ſide than on the oppoſite Bohemian. Aſſemanni Sileſia, p. 335. Such branches as run from north-eaſt to ſouth-weſt have their weſtern covered with primordial ſtrata, and conſequently leſs ſteep. 4 New Roz. p. 157.

8° The Meiſſener in Heſſia is ſteeper on the north and eaſt ſides, which face the Warra, than on the ſouth and weſtern. 1 Bergm. Journ. 1789, p. 272.

9° The mountains of the Hartz and Habichtswald are ſteep on the ſouth and gentle on the northern ſides. Foſter, § 46.

10° The Pyrenees, which run from eaſt to weſt, are ſteeper on the ſouthern or Spaniſh ſide. Carbonieres XIII.

11° The mountains of Crim Tartary are gentle on the northern and ſteep on the ſouthern ſides. Foſter, *ibid.*

In Aſia.

12° The Ourals, which ſtretch from north to ſouth, are far ſteeper on the weſtern than on the ſouthern ſides. Herman Geologie, p. 90; and 2d Ural Beſchreib. p. 389.

13° The mountain of Armenie, to the weſt of the Ourals, is ſteep on its eaſt and north ſides, but gentle on the ſouthern and weſtern. 1 Pallas Voy. p. 277.

14° The Altaiſchan mountains are ſteep on their ſouthern and weſtern ſides, but gentle on the northern and eaſtern. Foſter, *ibid.* and Herman 2d Ural Beſchreib. p. 390 in the note.

15° So alſo are the mountains of Caucaſus. 3d Schrift. Berl. Geſelſch. 471.

16° The

16° The mountains of Kamtſkatka are ſteep on the eaſtern ſides. Pallas, 1 Act. Petropol. 1777, p. 43.

17° The Ghauts in the Indian peninſula are ſteep on the weſtern ſides.

18° The mountains of Syria, which run from north to ſouth, ſkirting the Mediterranean, are ſaid to be ſteeper on the weſtern ſide facing the Mediterranean. 4 La Metherie, p. 380.

In America.

The Cordelieres run from north to ſouth; their weſtern flanks towards the Pacific are ſteep, their eaſtern deſcend gradually.

In Guiana there is a chain of mountains that run from eaſt to weſt; their ſouthern flanks are ſteep, their northern gentle. Voyages de Condamine, p. 140.

To aſſign the cauſes of this almoſt univerſal allotment of unequal declivities to oppoſite points, and why the greateſt are directed to the weſt and ſouth, and the gentleſt, on the contrary, to the eaſt and north, it is neceſſary to conſider,

1° That all mountains were formed while covered with water.

2° That the earth was univerſally covered with water at two different æras, that of the Creation, and that of the Noachian deluge.

3° That in the firſt æra we muſt diſtinguiſh two different periods, that which preceded the appearance of dry land, and that which ſucceeded the creation of fiſh, but before the ſea had been reduced nearly to its preſent level: during the former, the primæval mountains were formed, and during the laſt, moſt of the ſecondary mountains and ſtrata were formed.

4° That all mountains extend either from eaſt to weſt, or from north to ſouth, or in ſome intermediate direction between theſe cardinal points, which need not be particularly mentioned here, as the ſame ſpecies of reaſoning muſt be applied to them, as to thoſe to whoſe aſpect they approach moſt.

Theſe preliminary circumſtances being noticed, we are next to obſerve that, during the firſt æra, this vaſt maſs of

water moved in two general directions, at right angles with each other; the one from eaſt to weſt, which needs not to be proved, being the courſe of tides which ſtill continue, but were in that ocean neceſſarily ſtronger and higher than at preſent; the other from north to ſouth; the water tending to thoſe vaſt abyſſes then formed in the vicinity of the South Pole, as ſhown in my former eſſays. Before either motion could be propagated, a conſiderable time muſt have elapſed.

Now the primæval mountains formed at the commencement of the firſt æra, and before this double direction of the waters took place, muſt have oppoſed a conſiderable obſtacle to the motion of that fluid in the ſenſe that croſſed that of the direction of theſe mountains. Thus, the mountains that ſtretch from north to ſouth muſt have oppoſed the motion of the waters from eaſt to weſt; this oppoſition, diminiſhing the motion of that fluid, diſpoſed it to ſuffer the earthy particles, with which in thoſe early periods it muſt have been impregnated, to cryſtalliſe or be depoſited on theſe eaſtern flanks, and particularly on thoſe of the higheſt mountains, for over the lower it could eaſily paſs: theſe depoſitions, being inceſſantly repeated at heights gradually diminiſhing as the level of the waters gradually lowered, muſt have rendered the eaſtern declivities, or deſcent, gentle, gradual, and moderate; while the weſtern ſides, receiving no ſuch acceſſions from depoſitions, muſt have remained ſteep and craggy.

Again, the primæval mountains that run from eaſt to weſt, by oppoſing a ſimilar reſiſtance to the courſe of the waters from north to ſouth, muſt have occaſioned ſimilar depoſitions on the northern ſides of theſe mountains, againſt which theſe waters impinged, and thus ſmoothed them.

Where mountains interſect each other in an oblique direction, the north-eaſt ſide of one range being contiguous to the ſouth-weſt flanks of another range, there the afflux of adventitious particles on the north-eaſt ſide of the one muſt have frequently extended to the ſouth-weſt ſide of the other, particularly if that afflux were ſtrong and copious: thus the *Erzgebirge* of Saxony, which run from weſt to eaſt, have their north-eaſt ſides contiguous to the ſouth-weſt ſide of the *Rieſengebirge* that ſeparate Sileſia from Bohemia; and hence

theſe latter are covered with the ſame beds of gneiſs, &c. as the northern ſides of the Saxon, and thereby are rendered ſmooth and gentle comparatively to the oppoſite ſide, which, being ſheltered, remains ſteep and abrupt; which explains the ſeventh obſervation.

The cauſes here aſſigned explain why the covering of adventitious ſtrata on the higheſt mountains is generally thinneſt at the greateſt height, and thickeſt towards the foot, of the mountain; for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the maſs of earthy particles with which it was charged being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreaſed, the depoſitions from it on the higher parts of the mountains muſt have been leſs copious than on the lower, where they muſt have been oftener repeated.

Hence, 2°, granitic mountains, generally the moſt antient, frequently have their northern or eaſtern ſides covered with ſtrata of gneiſs or micaceous ſchiſtus, and this often with argillite, or primæval ſandſtone or limeſtone, theſe being either of ſomewhat later formation, or longer ſuſpendible in water.

Hence, 3°, different ſpecies of ſtone are often found at different heights of the ſame flank of a mountain, according as the water which conveyed theſe ſpecies happened to be differently impregnated at different heights: during the firſt æra its depoſitions formed the primitive ſtony maſſes; but after the creation of fiſh, limeſtone, ſandſtone, farcilites, and ſecondary argillites, in which piſcine remains are found, were depoſited. But, during the ſecond æra, *viz.* that of the Noachian deluge, by reaſon of the violence and irregularity of its aggreſſion, the depoſitions were more miſcellaneous, and are found at the greateſt heights; yet, in general, they may well be diſtinguiſhed by the remains of land animals or of vegetables, or of both, which they preſent in their ſtrata (or at leaſt by the impreſſions of vegetables which they bear), as theſe muſt have been conveyed after the earth had been inhabited. But mountains regularly ſtratified bearing ſuch remains,

mains, for inſtance the carboniferous, cannot be deemed to have been formed in a period ſo tumultuous. During this deluge the waters alſo held a different courſe, proceeding at firſt from ſouth to north, and afterwards in both oppoſite directions in our weſtern hemiſphere, as ſhown in treating of that cataſtrophe in my ſecond eſſay.

Hence, and from various contingent local cauſes, as partial inundations, earthquakes, volcanoes, the eroſion of rivers, the elapſion of ſtrata, diſintegration, the diſruption of the lofty mounds by which many lakes were antiently hemmed in, ſeveral changes were produced in particular countries that may at firſt ſight appear, though in reality they are not, exceptions to the operation of the general cauſes already ſtated.

Thus the mountains of Kamſkatka had their eaſtern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meiſſener had its eaſt and north flanks undermined by the river Warra, as Werner has ſhown: thus the eighth and ſixteenth obſervations are accounted for, as is the thirteenth, by the vaſt inundations ſo frequent in this country, 1 Pallas, p. 172, which undermined or corroded its eaſt ſide, while the weſtern were ſmoothed by the calcareous depoſitions from the numerous rivers in its vicinity.

Hence, 4°, we ſee why on different ſides of lofty mountains different ſpecies of ſtones are found, as Pallas and Sauſſure have obſerved, 2 Sauſſ. § 981; a circumſtance which Sauſſure imagined almoſt inexplicable, but which Dolomieu has ſince happily explained, by ſhowing that the current which conveyed the calcareous ſubſtances to the northern, eaſtern, and north-eaſtern ſides of the Alps, for inſtance, was ſtopped by the height of theſe mountains, and thus prevented from conveying them to the ſouthern ſides; and thus the north-eaſtern ſides were rendered more gentle than the oppoſite. 3 New Roz. p. 425, conformably to the theory here given.

Hence, 5°, where ſeveral lofty ridges run parallel to each other it muſt frequently happen that the external ſhould intercept the depoſitions that do not ſurmount them, and thus leave the internal ridges ſteep on both ſides.

Hence,

Hence, 6°, low granitic or other primitive hills are frequently uncovered by adventitious ſtrata on all ſides, as at Phanet in the county of Donegal, or are covered on all ſides; the impregnated waters either eaſily paſſing over them, or ſtagnating upon them, according to the greater or leſſer rapidity of its courſe, and the obſtacles it met with.

The two-fold motion of the antient ocean is noticed both by Buffon and Bergman, but neither of them has deduced from it the true explanation of the phænomena of which we here treat. Buffon attributes the formation of ſecondary mountains to depoſition or ſediments from the ſea after the exiſtence of fiſh, 1 Epoques, p. 143, in 8vo., which, he ſays, inveſted the baſes of mountains, without noticing any diſtinction of ſides, p. 144 and 170. He thinks theſe ſediments were equally conveyed from both poles towards the equator; for it is in the equatorial regions that, he thinks, thoſe mighty caverns opened, towards which the primitive ocean was impetuouſly borne, and in which it was ingulphed; p. 181, 182, and 183. If ſo, ſimilar declivities ſhould be formed on the ſouthern as on the northern ſides of mountains; which is contrary to the obſerved facts. His explanation of the eaſtern and weſtern declivities is defective and erroneous; for he attributes the abruptneſs of the weſtern ſides to the eroſion of the coaſts on that ſide (an eroſion that exiſts only in fancy), and the ſmoothneſs of the eaſtern, to the gradual deſertion and retreat of the ſea on that ſide, p. 184 and 185; a retreat equally fictitious, as De Luc has well ſhown. Whereas, ſince the general motion of the ſea is from eaſt to weſt, if the eroſion were of either ſide, it ſhould rather be on the eaſtern than on the weſtern; beſides, if the gentle declivities of the eaſtern ſides of mountains aroſe from the gradual retreat of the ſea, the petrifactions of the ſecondary mountains thus formed ſhould conſiſt of ſuch ſhell-fiſh as inhabit ſhallow ſeas or ſhores, whereas they conſiſt chiefly of thoſe called *pelagicæ*, which inhabit the greateſt depths *.

With reſpect to the eaſtern and weſtern declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2 Bergm. Erdeklotet, § 183 and 187;

* 2 Bergm. Erdekugel, p. 315.

but

but he fails in accounting for the inequality of the northern and ſouthern, for he ſuppoſes the courſe of the waters to tend equally from both poles towards the equator; which would render the depoſitions equal on both ſides; which is contrary to obſervation.

VI. *On the Identity of the Pyromucous, Pyrotartareous, and Pyroligneous Acids; and the Neceſſity of not conſidering them any longer as diſtinct Acids.* By C. FOURCROY *and* VAUQUELIN *.

I. *On the Multiplicity of the Vegetable Acids.*

WHEN Bergman and Scheele had diſcovered ſeveral vegetable and animal acids before unknown, and had deſtroyed the pretended identity admitted in regard to thoſe bodies, all the chemiſts employed themſelves with great zeal in ſubjecting various vegetable acid ſubſtances to a new examination. The number of compounds, which till then had been reduced to two kinds, increaſed ſo much, that it was ſuppoſed it would far exceed that of the acids which had been diſcovered among the foſſils, and that it would be impoſſible to ſet bounds to the multiplicity of theſe natural productions of organiſed bodies.

The four liquors, extracted by diſtillation from mucous bodies, wood, and tartar, though owing to an artificial decompoſition, have been comprehended in the claſs of the vegetable acids, well characteriſed as poſſeſſing a peculiar nature: ſome analogies founded on their origin, their brown colour, and their burnt odour, have cauſed them to be referred to a ſpecial genus, under the name of the empyreumatic acids. If we collect the facts which ſeem to prove that they are different in nature not only from the other acids, but from each other, they will eaſily ſhow that theſe acids have not been ſufficiently examined to be well known; that chemiſts had only very imperfect ideas reſpecting their nature; and that it was for want of accurate analyſis that

* From the *Annales de Chimie*, No. 104.

they

they were thought to be different from each other, and different in their whole nature from any other vegetable acid.

II. *Characters given to the Pyromucous Acid.*

Lemery, Boerhaave, Neumann, Cartheuſer, Macquer, and Bucquet, were the firſt who inſiſted on the ſpecific characters of that acid which has been called the pyromucous in the Methodical Nomenclature, and which had long been diſtinguiſhed by the name of *ſpirit of honey*, of *ſugar*, of *manna*, of *gum*, &c. M. Schrikel, in giving a detailed hiſtory of the analyſis of ſugar and of this acid in 1776, greatly enlarged and confirmed the ideas before conceived reſpecting the peculiar nature of the pyromucous acid, and all chemiſts have ſince adopted that opinion. It was characteriſed by its dark red colour when impure, and its golden yellow colour after being rectified; by its odour of caramel, its very pungent taſte, its volatility, nearly ſimilar to that of water; by its action on the ſkin, which it tinges red, and on the glaſs of retorts, which it ſeems to attack. It was believed to be entirely contained in mucous matters, and to be only developed by fire; it was ſaid to be ſuſceptible of decompoſition by heat, and of being converted into gaſeous bodies. But, beſides that its ſuppoſed action on glaſs depended only on the adherence and penetration of the charcoal of the ſugar; beſides the proof, already well eſtabliſhed, of its non-exiſtence in ſaccharine and gummy matters, and of its entire production by fire, it muſt be allowed that, in examining what has been done in regard to ſaline compounds and the attractions of the pyromucous acid, no reſult had been obtained but vague and undetermined notions, which ſeemed to excite doubt and uncertainty in regard to the particular nature of this acid. The reader will be enabled to judge of the truth of this aſſertion by peruſing, in particular, the details given under the article *Syrupy Acid* of the *Dictionnaire de Chimie Encyclopédique*, where our learned colleague Guyton has collected every thing then known reſpecting this empyreumatic acid: when drawing up, therefore, the article in my Syſtem of Chemical Knowledge, written and printed ſeveral months before the diſcovery now ſubmitted to the Inſtitute, I was

not able to obtain the real diſtinguiſhing characters of the pyromucous acid.

III. *Characters aſcribed to the Pyrotartareous Acid.*

We are indebted to Lemery and Neumann for our firſt knowledge of this acid, furniſhed by diſtilled tartar, and different from the tartareous acid. They found that tartar, by diſtillation, gave the fourth of its weight of acid phlegm. According to the preſcription of Neumann, it was to be rectified at a gentle heat; but C. Guyton was not able to perform that operation with ſucceſs, and the retort was always broken by the exploſion which took place. The empyreumatic odour and taſte of this acid are the two moſt ſtriking characters which made it be admitted by chemiſts as a peculiar acid. It is difficult to conceive why Venel ſhould take this acid product of diſtilled tartar to be nitric acid, which he thought might be extracted alone, and in a ſtate of purity. It is no leſs aſtoniſhing to hear C. Monnet maintain, in conſequence of experiments made at ſome length, that diſtilled acid of tartar is muriatic acid, though he really found in it, according to his own acknowledgement, only approximative characters. The academicians of Dijon and C. Berthollet, after repeating all the experiments of Monnet, were convinced that there is no real analogy between the acid product of diſtilled tartar and the muriatic acid. Scheele, in proving that there is always a little real muriatic acid in the alkali of tartar, inſiſted on the differences between this acid and that of tartar. M. Fontana proved that the acid diſtilled from tartar can be entirely reſolved into carbonic acid gas and carbonated hydrogen gas; ſo that, by bringing it nearer to all the other vegetable acids, he removed further every idea of confounding it with the mineral acids. In the laſt place, Guyton, in the firſt part of the firſt volume of the *Dictionnaire de Chimie Encyclopédique*, publiſhed in 1786, after detailing all the different opinions as well as the principal facts known reſpecting this acid, concluded that it would be neceſſary to conſider it as a peculiar acid; a product of tartar altered by heat, diſtinguiſhed from the latter by its being incapable of cryſtalliſation, and by the ſoluble ſalt which it

forms with lime; and he called it the tartareous empyreumatic acid. Theſe ideas, adopted in 1787 by the authors of the Methodical Nomenclature, induced them to diſtinguiſh this acid by the name of the pyrotartareous acid. Since that period, chemiſts have continued to conſider this acid as a diſtinct ſpecies, and no one has ſubjected it to a new examination.

IV. *Characters given to the Pyroligneous Acid.*

Boerhaave firſt made known, under the name of *acid ſpirit of wood*, the product diſtilled from woody bodies; and he even compared it to a kind of vinegar. M. Goetling has given a particular hiſtory of it in Crell's Chemical Journal for the year 1779. Guyton made it known afterwards, under the name of the ligneous acid, in the firſt part of the *Dictionnaire Encyclopédique*, publiſhed in 1786. He collected all the information then acquired reſpecting this acid, according to the experiments made by the Academy of Dijon. Every kind of wood examined has hitherto furniſhed by diſtillation the ſame acid; the difference in regard to colour, ſavour, acrid odour, and particularly that of ſaline compounds, formed by the liquid diſtilled from wood, have made it to be diſtinguiſhed from every other acid: in the Methodical Nomenclature it has been called the pyroligneous acid, and in my Syſtem of Chemiſtry I claſſed it with the pyromucous and pyrotartareous acids, as forming with them a kind of acids compoſed of ſpecies analogous to each other in the ratio of their identic origin, and of ſome comparative properties manifeſtly depending on their empyreumatic ſtate. The characters adopted to diſtinguiſh the pyro-ligneous acid were, its ſmoky ſmell, its reddiſh colour, its property of giving a laſting ſtain to wood, the ſalts which it formed with alkaline baſes, and the elective attractions which it obeyed. In giving a ſyſtematic explanation of the properties of this acid, as well as in regard to thoſe of the pyromucous and pyrotartareous acid, the want of more accurate knowledge was always felt; characters ſufficiently ſtriking and well defined, proper to diſtinguiſh theſe three acids from each other, could not be found; and even after they had been claſſed together into a diſtinct genus, on account of their empyreu-

matic nature, which gave them a ſtriking reſemblance, when chemiſts wiſhed to explain what were the peculiarities of each ſpecies, they were obliged to acknowledge that the examination of theſe productions of fire had not been carried to a ſufficient length, and that their properties had not been ſo thoroughly ſtudied as to enable them to aſſign characters ſufficiently ſtriking, and to give a correct hiſtory of them.

V. *Firſt View of the Acetous Nature of the Empyreumatic Acids.*

Such was the uncertain ſtate of the ſcience in regard to the three acids extracted from vegetable matters by fire, when a circumſtance, preſented, as it were, accidentally, gave us an opportunity of diſcovering a new fact reſpecting their nature. In the courſe of thoſe reſearches in which C. Vauquelin and myſelf have been ſo long engaged reſpecting the chemical analyſis of vegetable ſubſtances, and of the experiments which we made on the ſolid or ligneous parts of plants, we were employed ſome months in the examination of cork. This epidermal covering of a ſpecies of oak had appeared to me, for ſeveral years, to be of a nature ſo very diſtinct from other vegetable tiſſues as to deſerve a particular reſearch: ſeveral experiments on the epidermis of other trees had inclined me to think that in their characters they approached near to cork, and I thought it my duty to preſent it as one of the immediate materials of plants under the name of *ſuber*. Being deſirous of carrying my firſt eſſays ſtill further, and of giving more extent to theſe firſt ideas, we began to employ ourſelves this ſummer on a more exact analyſis of *cork*. Having ſubjected a pretty large quantity of it, 0·61lb. (three hectogrammes), to diſtillation with a naked fire, we obtained from it a fourth of its weight of a very light and volatile oil, and almoſt the ſame quantity of a reddiſh empyreumatic liquid, which exhibited all the apparent characters of the pyroligneous acid. But this acid liquor, when examined with more care, rectified and combined with alkalies, and diſengaged from its baſes by weak ſulphuric acid, ſoon exhibited properties of real acetous acid, and, after having ſeparated it by ſlow diſtillation from the portion of coloured oil which

which it held in ſolution, it evidently appeared to be that acid.

When this fact was once aſcertained, it was not difficult for us to preſume that the pyroligneous acid, from whatever wood extracted, could only be acetous acid: we had even reaſon to ſuſpect that the two other empyreumatic acids were of the ſame nature; but as a conjecture, however well founded, has no real value in phyſics, we called in the aid of experiment to enable us to diſcover the truth, or to reject our opinion as an error.

VI. *New Examination of the Pyromucous, Pyrotartareous, and Pyroligneous Acids.*

1. Having diſtilled, with great caution, ſixteen parts of pure ſugar, which furniſhed us with ten parts and a half of water charged with reddiſh pyromucous acid, a little more than four parts and a half of light charcoal, and half a part of gas, we combined this acid liquor with lime; the liquor was then evaporated to dryneſs, and afterwards treated in a retort with weak ſulphuric acid. We thus obtained a pretty thick, clear, or very little coloured liquid, having a very ſenſible acetous ſmell, and which, when combined with potaſh, gave a very evident acetite of potaſh. This ſalt, indeed, had a dirty gray colour; but, by filtering a warm ſolution of it through pounded charcoal, it loſt the oil which coloured it, and became quite clear.

The acid product, obtained from the firſt calcareous ſalt by the ſulphuric acid, was already much leſs coloured than the pyromucous acid; it now retained only very faintly the ſmell of caramel (pan ſugar), which characteriſes the acetous acid.

When diſengaged a ſecond time from alkaline ſalt by the ſulphuric acid, it was ſtill purer, had nothing of its firſt origin, and now exhaled only a pure acetous odour. We had reaſon, therefore, to conclude, from theſe experiments, that the pyromucous acid was nothing elſe than acetous acid united to an empyreumatic oil ariſing from the decompoſition of the ſugar by fire.

The ſame reſult took place with acid liquors extracted from

from gums, honey, manna, ſtarch, paper, and ſeveral other vegetable ſubſtances which are known to yield pyromucous acid by diſtillation.

2. White and puriſied tartar gave by the retort nearly a half leſs of acid liquor, than ſugar furniſhed of pyromucous acid. This tartareous acid product, obtained by a well regulated heat, was almoſt tranſparent, and had not the reddiſh-brown colour of the empyreumatic acids extracted by means of a ſtrong heat. It however had an acrid empyreumatic odour, a heavy and highly coloured oil floated over it, and, notwithſtanding its pungent nature, it would have been difficult to aſcertain it to be acetous acid by this ſingle experiment: but it did not long deceive us. Having ſaturated it with potaſh immediately, on account of its little colour and impurity, we diſtilled it, after being evaporated to dryneſs, with diluted ſulphuric acid, and it furniſhed acetous acid, eaſy to be diſtinguiſhed as ſuch, without any mixture of empyreuma.

We found that by diſtilling pyrotartareous acid, even not rectified and in a red ſtate, from off pounded charcoal, previouſly well lixiviated and carefully dried, it loſt by this ſimple operation its empyreumatic nature: we are even perſuaded that mere filtration of this acid warm, through charcoal, ſeveral times repeated, would be ſufficient to ſeparate from it the oil, and to bring it to the ſtate of acetous acid almoſt pure.

No doubt, therefore, can remain, that the pyrotartareous acid is acetous acid rendered impure by a portion of empyreumatic oil produced by the decompoſition of the tartareous acid by caloric.

3. The pyro-ligneous acid obtained from ſhavings of beech wood, diſtilled with care, formed with lime a brown ſalt not cryſtalliſable. This ſalt, when treated with very weak ſulphuric acid, gave acetous acid, known by its ſmell, the deliqueſcent acetite it formed with potaſh, or the cryſtalliſable acetite obtained from it with ſoda.

Another portion of the ſame primitive pyroligneous acid, when immediately united to potaſh, and filtered in its ſolution through powdered charcoal, gave an acetite of potaſh almoſt colourleſs, from which weak ſulphuric acid diſengaged,

by

by diſtillation, very pure acetous acid, almoſt without empyreumatic odour or fumes.

It muſt here be obſerved that the pyroligneous acid, that of the three acids obtained by fire which has the moſt ſtriking empyreumatic odour and character, is alſo one of the three moſt difficult to be purified, and to be carried to the ſtate of very pure vinegar. It does not, therefore, aſſume the nature of the laſt, as the tartareous acid does, by filtration alone, nor even by two ſucceſſive diſtillations from off powdered charcoal. By employing even the aid of ebullition and ſtrong agitation with charcoal, it cannot be deprived of its oil, while both of theſe proceſſes ſucceed with certainty and eaſe in purifying the pyromucous acid, which, indeed, gives up with moſt eaſe its oil; and even in purifying the pyrotartareous acid, though it be a little more difficult to ſeparate its oil than that of the pyromucous acid.

But though it is more refractory in oppoſing purification, and that kind of analyſis of which I here ſpeak, it is no leſs proved, that the pyroligneous acid, like the two preceding, is only acetous acid impregnated with empyreumatic oil, produced from wood altered by heat.

VII. *Artificial Converſion of the pure Acetous Acid into the Pyromucous, Pyrotartareous, and Pyroligneous Acids.*

The preceding experiments might be ſufficient to make us acquainted with the identic and real acetous nature of the three empyreumatic acids, which have been hitherto conſidered as forming three diſtinct acids, and belonging to a genus well characteriſed. But to theſe experiments a ſupplement was ſtill wanting; it was neceſſary to inquire whether it was not poſſible to imitate theſe acids with that of vinegar, by adding what ſeemed to be added to it in each of theſe acids produced by fire.

It was before fully proved that each of theſe products of diſtillation differed from the other two by an empyreumatic oil united to it by the effect of its diſtillation alone. It was very eaſy, therefore, to try to diſtil vinegar from off oils diſtilled from a mucilage, tartar, and wood. This trial was attended with all the ſucceſs that could be expected. Acid

of

of vinegar, heated in a retort with each of these three oils, furnished a coloured and odorous product exactly of the same nature as primitive pyromucous, pyrotartareous, and pyroligneous acids: the odour and smell of these acids were very perceptible; only these factitious empyreumatic acids were a little stronger and sourer than those arising from distillation, but nothing more was necessary to carry them to the same state of weakness than to add to them a little water.

By repeated trial we obtained a manner of forming with the acid of vinegar the three empyreumatic acids much speedier, and in a more simple manner, than by distillation. We found that it was sufficient to throw some drops of the empyreumatic oil of mucilages, of tartar, or of wood, into the acetous acid, to suffer them to remain some time together, or to shake them strongly, to imitate these acids made by fire. The oil almost immediately unites with the acid, dissolves in it, gives it a red or brown colour, and communicates to it, at the same time, the acrid odour and bitter pungent taste known in the pyromucous, pyrotartareous, and pyroligneous acids.

The acetous acid is then a real solvent of empyreumatic oils, and in that case it assumes the characters of the acid liquors or odorous spirits, as they were formerly called, which issue from vegetable matters treated by fire. To separate these oils dissolved in the acetous acid, and to bring back the latter to its purity and to its primitive simplicity, the same means must be employed as for freeing the acetous acid from the empyreumatic acids united to it when obtained by distillation, *viz.* filtration, agitation, ebullition with charcoal, union with lime and alkalies, and the disengagement of these combinations by weak sulphuric acid.

It is to this solubility of oils in the acetous acid, that the odour which this acid so easily acquires and retains seems to be owing: it is to it that we must ascribe the distinction of a great number of empyreumatic acids, which several chemists have been induced to make, and which they could not avoid doing, while continuing to consider, in the acid products of distilled vegetable matters, the odour, colour, and savour, as characters proper, if not for positively ascertaining,

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at leaſt for conjecturing them to be acids different from each other; and eſpecially by comparing theſe characters in acid products furniſhed by kinds of wood very different in their properties, and conſequently in their products.

VIII. *Reflections on the Frequency and the Nature of the different Proceſſes which furniſh Acetous Acid.*

After having proved that mucilages, ſaccharine bodies, fæculæ, as well as wood and tartareous compounds, give by diſtillation real acetous acid, concealed in the products of each of theſe bodies by a portion of oil, having a peculiar odour, ſmell, and colour, and that, conſequently, we ought to deduct from the number of the vegetable acids theſe three empyreumatic acids, we thought it might be uſeful to ſcience to give ſome obſervations here on the production of the acetous acid. The knowledge which the chemical art poſſeſſes at preſent reſpecting this production, though much more extenſive than formerly, has never yet been collected into any work; it may, however, be of great importance in vegetable analyſis: ſuch is the motive which induced us to preſent a ſhort view of it, without, however, entering into all the details which the ſubject might require in a ſyſtematic work.

The formation of the acetous acid, which has always been believed to be neceſſarily produced by a fermentation peculiar to vinous liquors, is ſo frequent in the proceſſes of art and the operations of nature, that it is indiſpenſably neceſſary to make it a general phenomenon, and to ſtudy this phenomenon under the name of *acetification*, as proper to throw light on the chemical properties of organiſed bodies. We may conſider it either in regard to ſubſtances ſuſceptible of acetification, or in regard to circumſtances which favour transformation into acetous acid.

Under the firſt point of view, beſides vinous liquors, which were firſt found, and long thought to be, the only ones capable of acetification, we have found a multitude of bodies ſuſceptible of experiencing this change. There is ſcarcely any vegetable extract in which acetites are not found: ſap, if it has been kept only a few hours, contains ſome of it;

even different kinds of mould itself are charged with it, as may be easily proved by distilling them with a little diluted sulphuric acid: tan, when heated, emits an odour of vinegar, and furnishes some of it by the same treatment: water in which pulse, cabbages, carrots, turnips, potatoes, cucumbers, the pods of French beans, &c. have been steeped and grown sour, is exceedingly acetous: the water from the starch manufactories is the same: the juices of acid fruits, those of apples, pears, gooseberries, strawberries, raspberries, cherries, oranges, and lemons, when exposed some hours to the warm air, assume, along with a strong and pungent odour, a taste differently and more strongly acid than that which they had naturally; besides perceiving in them acetous acid, you will obtain it pure and insulated by subjecting these juices to distillation. It is well known from the experiments of Scheele, that milk in becoming sour gives acetous acid; we have found that bouilli and animal jelly form this acid also: in a word, we have said in other memoirs that the urine of the mammiferæ, and that of man in particular, had the property of becoming acetous, and gave a great quantity of very strong acid by distillation.

Thus the number of the substances susceptible of acetification is very considerable: extractive matter, mucilage, saccharine bodies, fæcula, and starch; even ligneous bodies, tan, the greater part of the primitive vegetable acids, the gelatin of animals, the caseous matter, and even *urée*, that body peculiar to urine, and which characterises it by its remarkable properties, all these products of vegetable and animal organisation and life are equally susceptible of acetification.

It is true, that the circumstances under which we have presented their conversion into acetous acid, seem all to belong to a fermentation, and that it might be thought that they follow a formation more or less striking or fugitive of vinous matter, but it remains for us to show that these matters may be acetified by phenomena or causes different from fermentation; and this fact is already proved by the acid productions of the distillation, which form the principal subject of this memoir. It is here seen that the action of fire really acetifies gums, mucilages, tartrites, wood. A know-

ledge of the chemical phenomena exhibited by vegetable ſubſtances in regard to their acidification in general, enables us to obſerve and diſtinguiſh four circumſtances which promote their acetification or converſion into acetous acid.

The firſt is, the decompoſing action of the fire in diſtillation. Without here entering into more extenſive details on this ſubject, which, when we conſider the object of this memoir, are leſs neceſſary, we ſhall content ourſelves with obſerving, that this action of caloric, by diſuniting the conſtituent principles of vegetable matters, combines a part of them in ſuch a manner as to give birth to acetous acid; and that this converſion is accompanied with the formation of water, the formation and diſengagement of gaſeous carbonic acid, and the precipitation of carbon in the ſtate of charcoal.

The ſecond mode of acetification of vegetable compounds is that ariſing from the action of powerful acids, and particularly of the ſulphuric, the nitric, and the oxygenated muriatic on theſe compounds. This kind of production takes place in gums, ſugar, extracts, and gelatin, when treated by acids; the greater part of the other vegetable acids, and even alcohol itſelf, often experience ſuch a change by the decompoſing influence of the acids above mentioned. While this kind of acetification takes place, there are formed alſo water and carbonic acid; ſometimes carbon likewiſe is depoſited. We muſt here add, that this acetification is the laſt term of vegetable acidification in general, ſince in treating acetous acid by the ſame decompoſing action of the mineral acids, you deſtroy its acetous nature, and make it paſs to the ſtate of water and carbonic acid, as is the caſe in every vegetable decompoſition carried to its *maximum*.

The third mode of acetification is the oldeſt known of the whole, and the only one formerly admitted: it is the acetous fermentation that converts all the different kinds of wine into vinegar: in this there is neither a precipitation of carbon, nor diſengagement of carbonic acid. It is well known that it takes place in conſequence of an abſorption of the oxygen of the atmoſphere, and that it ſuppoſes the preexiſtence of vinous liquors.

In the laſt place, we conſider as the fourth and laſt mode

of acetification, a kind of peculiar fermentation, which does not require the preſence of wine, which takes place in matters foreign to the nature of vinous liquors, and which has ſome relation to putrid decompoſition. It is that obſerved in animal liquors abandoned to themſelves, and particularly in urine.

Each of theſe modes of acetification, though equally giving birth to acetous acid, and ſuppoſing the ſame compoſition from which that acid ariſes, ſince it is the ſame in every caſe when it has been purified, admits, however, a difference in the products which accompany it. Each of the acetous acids ariſing from it, preſents indeed a ſpecific character proper for making it known and for indicating the ſource which has given birth to it.

Thus, 1ſt, The acetous acid obtained by fire is empyreumatic; it holds in ſolution an acrid oil, which gives it a peculiar odour, colour, and ſavour.

2d, Factitious acetous acid, produced by the action of other acids, is characteriſed by the preſence of the malic, or of the oxalic acid, formed at the ſame time as itſelf, and by its weakneſs, depending on the proportion of the water, which is alſo formed with the three preceding acids.

3d, The acetous acid ariſing from wine contains tartar, alcohol, and a colouring matter, which give it a peculiar character. It is, as has been ſaid, a ſpirituous acid.

4th, In the laſt place, the acetous acid produced by putrid fermentation is always united, in whole or partially, to ammonia, which, like it, ariſes from this ſeptic commotion.

But whatever may be the matters or new compounds united to the acetous acid formed under any of the four circumſtances abovementioned, this acid, capable of being ſeparated with greater or leſs eaſe from each of theſe ſubſtances, is always the ſame, and always ſimilar to that extracted from ſour wine by the help of diſtillation.

It muſt therefore be now admitted that the acetous acid is not the ſole and neceſſary product of the fermentation of wine, and that its production, exceedingly frequent, is one of the moſt conſtant phenomena of vegetable and animal analyſis.

VII. *Account*

VII. *Account of a fatal Accident which happened to a Traveller on the Glacier of Buet; with ſome Cautions to thoſe who through Curioſity may viſit the Mountains of Switzerland, and particularly the Glaciers. By* M. A. PICTET, *Profeſſor of Philoſophy* *.

A SENTIMENT of curioſity, exceedingly natural, induces travellers from all parts of Europe to viſit Mont Blanc, the higheſt point of the old world, and to examine the ſurrounding glaciers. Since the memorable aſcenſion of the learned hiſtorian of the Alps, theſe places have acquired a new degree of intereſt: the geologue, the mineralogiſt, and the mere amateur repair thither with avidity; and even women are amply indemnified for the fatigue of the journey by the pleaſure ariſing from the view of objects entirely new to them, and by the amiable and friendly reception they meet with from the inhabitants. Every thing unites to make this excurſion, which is attended with no real dangers, the uſual object of all curious travellers who viſit Geneva and its environs.

The more this journey preſents attractions, the more it is of importance to make known the dangers to which travellers who undertake it may be expoſed merely by imprudence or inattention. Our principal view is utility, and without doubt it is of utility to make known, in all places where this journal may circulate, dangers which are indeed great to thoſe ignorant of them or who forgot them, but of little importance to thoſe who are forewarned or cautioned to avoid them. Had we treated this ſubject ſome years ago, the fatal accident which lately took place, and to which we were almoſt witneſſes, would not, perhaps, have happened. This reflection will not allow us to heſitate any longer. The experience I have acquired by travelling among the mountains, either when accompanying my illuſtrious friend Sauſſure, or in ten journeys undertaken to the glaciers of Chamouni in particular, will perhaps entitle me to ſome confidence from thoſe whom I am deſirous of ſaving from uneaſineſs or

* From *Bibliotheque Britannique*, No. 112.

danger:

danger: I ſhall, however, refer to the end of this article the cautions in this reſpect which have been ſuggeſted to me by experience, and ſhall proceed to an account of that event which induced me to take up my pen on the preſent ſubject.

C. d'Eymar, præfect of Leman, an enlightened lover of the arts, and a paſſionate admirer of the beauties of nature, having lately propoſed to viſit the glaciers of Chamouni, a canton which at preſent forms the eaſtern boundaries of the department under his adminiſtration, invited me to accompany him; and I readily embraced his obliging offer. We ſet out on the 7th of Auguſt, and ſlept the firſt night at Sallenches, as travellers do in general.

Next morning, during our firſt hour's march, we met a young man on foot, accompanied by a peaſant who was carrying a valiſe. We were ſtruck with the melancholy and dejected air of this peaſant. When we arrived at Servoz, three leagues from Sallenches, we learned from Deville, a very intelligent and experienced guide who attended us, that the morning of the day before, a ſtranger, the companion of the young man we had met, being with his friend and a guide on the glacier of Buet, had ſuddenly diſappeared, at the diſtance of ſome paces before them, in a crevice of the glacier covered with ſnow, which had given way under his feet. When they reached the mouth of the crevice, the bottom of which they could not perceive, the two ſurvivors called out a great number of times, but in vain, to their unfortunate companion ſwallowed up in the abyſs; and they did not quit the place till they had loſt all hope of his ſafety. M. Zimpfen, the young man whom we met, when he arrived at Servoz, had given Deville a commiſſion in writing, to endeavour, if poſſible, to diſcover the body of M. Eſchen, his friend, and to cauſe it to be interred.

As ſcarcely twenty-four hours had elapſed ſince the event, the ſenſible mind of d'Eymar was ſtruck with a ray of hope, and he immediately and officially enjoined Deville (for he heſitated, and not without reaſon) to furniſh himſelf with the neceſſary means, to ſet out in the utmoſt haſte, accompanied by ſuch a number of men as he preſumed might be neceſſary, and to give him an account of whatever ſhould be the

reſult.

reſult. It was at leaſt a journey of nine hours from Servoz to the glacier in queſtion; and as this glacier did not form any part of thoſe we intended to viſit, it was impoſſible for us to be informed of the iſſue of the propoſed ſearch till we paſſed Servoz on our return from Chamouni, to which we continued our route not without dejection.

It is with regret that I am obliged to omit the details of the ſtay we made in this intereſting valley; they have a character which will always be imprinted in my remembrance, but they would be foreign to the preſent object. However, while the brave Deville and his companions are engaged with their enterpriſe, I ſhall venture to ſuſpend the impatience of my readers for a few moments in order to make them acquainted with the glacier of Buet, and the motives which might induce travellers to viſit it.

It is to Meſſrs. De Luc, the two brothers, that philoſophers and naturaliſts are indebted for diſcovering the poſſibility of reaching this ſummit covered with eternal ſnow. It is an inſulated mountain ſituated in front of the central chain to which Mont Blanc and its glaciers belong, and ſeparated from them by a lower chain that runs in a parallel direction. This glacier may be ſeen from Geneva immediately on the left of the Môle. It appears under the form of a ridge not very ſalient, and which ſeems to be eaſy of acceſs. Meſſrs. De Luc were, however, deceived in this reſpect, and the account of three attempts they made to reach it, the laſt of which alone, on the 20th of September 1777, was attended with ſucceſs, is one of the moſt intereſting epiſodes to be found in the works of any naturaliſt. It was a deſire to diſcover the law which the decreaſe of heat in boiling water follows in proportion as it is raiſed in the atmoſphere, that induced them to viſit this mountain, and to brave, three different times, difficulties and dangers of various kinds to which they were expoſed in attempting to reach a ſummit that may be conſidered as the utmoſt poſſible boundary of philoſophical obſervation. Honour to that ſcience which inſpires courage ſo perſevering, and which produces it in ſucceeding generations! Our daring countrymen never ſuſpected that, ſeventeen years after this expedition, Sauſſure

would

would repeat their experiment on Mont Blanc itſelf, that is to ſay, about 850 toiſes higher than they were able to aſcend after great danger and fatigue.

Some years ago, a much eaſier route for arriving at the ſummit of Buet or La Mortine (for it is known by that name alſo) than that followed by Meſſrs. De Luc was diſcovered, and by this route I aſcended it twice without experiencing any difficulty. You firſt ſleep at Chalets de Villy, the laſt place of paſture of the valley which begins at Servoz and terminates at the glacier of Buet. From Villy you proceed to the Col de Salenton by a path practicable for mules; you then encounter the mountain on its eaſtern and ſouthern ſides, and, paſſing alternate declivities of ſnow and ſlate, reach the ſummit at the end of two hours and a half. The mean of two obſervations of the barometer which I made there, and which were very little different in their reſults, gave me 1594 toiſes for its height above the level of the ſea.

The mountain itſelf preſents nothing very intereſting in a lithologic point of view; it conſiſts of ſlate intermixed with veins of rotten quartz, or quartz ſimilar to ſtalactites; but as a belvidere, nothing in my opinion exiſts that can be compared to it. You here embrace at one view the whole ſpace comprehended between Jura on the weſt, as far as the mouths of the Rhone on the eaſt, and, on account of this circumſtance alone, I conſidered this mountain as exceedingly proper for receiving ſignals in an intended meaſurement of a degree of latitude and two degrees of longitude in the parallel of Geneva; a plan which may be found detailed in a memoir publiſhed in the Philoſophical Tranſactions of the Royal Society of London for the year 1791 *.

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* I cannot convey a better idea of the ſituation of this ſpot than by quoting the words of M. De Luc:—"It is difficult to give a deſcription in words when they do not awaken ſenſations which have been felt; I do not, therefore, flatter myſelf with the hope of exciting in the minds of my readers thoſe which I then experienced. The moſt profound ſilence prevailed in theſe regions; we perceived that they were not made for living beings; they were as little known to our guides as to us. The Chamois goats never approach them, and conſequently no hunter had ever aſcended ſo far.

"This

In my ſecond journey to mount Buet, I was under the diſagreeable neceſſity of being conſtantly enveloped by clouds

" This ſenſation of profound ſolitude was one of thoſe which we could moſt eaſily explain, but it does not explain our ſituation. We were on an immenſe extent of ſnow, the whiteneſs of which nothing altered. The rays of the ſun, reflected in a ſtraight line from the ſnow towards that luminary, ſhowed us how ſmooth it was; and this ſmoothneſs the imagination extended to every thing around. We ſaw nothing but this ſnow, and the heavens, towards which it was terminated in various folds, delicately rounded like thoſe beautiful argentine clouds which are ſometimes ſeen floating majeſtically in the pure atmoſphere. This was exactly what produced the extraordinary ſenſation which we then experienced. We actually thought ourſelves ſuſpended in the air on one of theſe clouds.—And what kind of air? Never had we before ſeen it of ſuch a colour. It was blue, dark and bright at the ſame time, which produced an inexpreſſible ſenſation of immenſity.

" It was near noon when we arrived; and, on raiſing our heads above the veil which ſo long concealed from us the eaſtern part of the horizon, we ſuddenly diſcovered the immenſe chain of the Alps in an extent of more than fifty leagues. On whatever ſide we turned our eyes, the whole horizon was covered with mountains. Its boundary on the weſt was nothing but the thickneſs of the air; for we overlooked the chain of Jura, diſtant about thirteen or fourteen leagues, ſo much that we could have ſeen the plains of Franche-Comté and Burgundy, if the air had been ſufficiently tranſparent. On the ſouth-weſt our view extended as far as Mount Cenis, and on the north-eaſt probably as far as Saint Gothard. We were far raiſed above all the defiles of the Alps, and only a few of their peaks were elevated above us.

" In all this vaſt ſpace, where mountains were accumulated on each other, we could diſcover no plain but in a ſmall corner to the weſt, the middle of which was occupied by Geneva; and on the north-eaſt we ſaw, almoſt from one end to the other, the large valley through which the Rhone flows, from the place where it falls from the mountains, as far as Sion, the capital of the Valais, diſtant from the place where we were nine or ten leagues. All the reſt was filled with mountains.

" The details as well as the *enſemble* would have excited the attention of the moſt indifferent beholder. A ſingle view of the immenſe quantity of ice and ſnow which covers the Alps will be ſufficient to ſatisfy the ſpectator reſpecting the duration of the Rhone, the Rhine, the Po, and the Danube. It inſpires us with an idea that this is their common reſervoir, and that it is ſufficient to ſupply them with water during ſeveral years of drought. We compared, without having need of calculation, their ſtreams with their ſources. Theſe ſources appeared to us only ſmall

clouds during the ſix hours I remained on it. I was exceedingly cold, and, in order to warm ourſelves, I and my guides were obliged to conſtruct a hut on the ridge of rocks neareſt the ſummit. We had at hand large pieces of ſlate, and our building was ſo ſolid that it ſtill exiſts, and has ſheltered more than one curious traveller from the ſeverity of the weather.

The glacier, which covers this ſummit, differs from the greater part of thoſe accumulations of ice known under that name in this reſpect, that the latter generally occupy the valleys, or the defiles in which the ice has not been originally formed, but to which it has ſunk down by its own weight, and the preſſure of the ice above it; whereas the ice of Buet has been formed in the place where it exiſts; and at that height in our parallel the ſnow never melts in ſummer. This mountain, therefore, may ſerve to determine with ſome preciſion the loweſt boundary of the ſnow in our climates.

Thus, for example, by obſerving from Geneva, through a teleſcope furniſhed with a micrometer, the vertical angle comprehended between the ſummit of the glacier and the loweſt boundary of the ſnow, I found it to be 16′ 14″, which, taking the diſtance of Buet from Geneva at 29820 toiſes, correſponds to 141 toiſes, the diſtance of this boundary below the ſummit, which places it at 1453 toiſes above the level of the ſea.

It may here be aſked, what is the mean annual temperature at this height in our latitude? We have pointed out ſomewhere in our Journal a very ſimple formula, which Sauſſure deduced from a great number of obſervations, and which repreſents very well the law of the decrement of the mean heat of the atmoſphere from the bottom upwards. This decrement is a hundredth part of a degree of the thermometer of Reaumur per toiſe of perpendicular elevation. This for-

rills, when compared with the valleys filled with ice, from which they flowed. Mont Blanc, which roſe above theſe valleys, ſeemed capable of furniſhing alone, for a long time, a ſufficiency of water for a river, ſo much was it loaded with ice from the top to the bottom; that is to ſay throughout a prodigious extent.—*Recherches ſur les Modifications de l'Atmoſphere*, vol. ii. p. 390.

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mula, therefore, applied to the mean temperature at the level of the sea, in the parallel of 46° (the latitude of Buet), as established in Dr. Kirwan's Work on the Temperature of the Globe, viz. 56·4° of Fahrenheit, or 10°,8 of Reaumur, will give for 1453 toises 14·53 degrees to be deducted; which makes the mean annual temperature of the lower boundary of the snow, in that parallel, to be 3·63 degrees below zero *.

It is not surprising, therefore, that this mountain should be crowned with a glacier, since the snow which falls there during the cold season, never entirely melts in the summer. The water produced by the partial melting of the surface of the snow filters through the porous snow beneath it, and freezing in its interstices gradually converts it into ice. In this manner is formed an accumulation, the thickness of which M. De Luc endeavoured to estimate from the following observation:

"We judged," said he, "by the position of these small rocks, about 200 feet lower than the highest part of the ice, that they formed a part of the real summit of the mountain. The whole mass above them was nothing but ice, in the form of a cone, cut through its axis, 200 feet in height, with a very extensive base, and resting on the im-

* I had occasion not long ago to discuss this formula with a philosopher, who observed to me, that, according to its nature, it was impossible it could be correct; because the density of the air, an element on which the preservation of heat in the different strata of the atmosphere essentially depends, decreases in geometrical progression; while the heights in toises, which represent the temperatures, proceed in arithmetical progression. I admitted the justness of the observation, speaking mathematically; but, in a physical point of view, as the formula is composed of co-efficients, some of them unknown or inappreciable, which gives to the temperature an arithmetical progression, decreasing from the bottom upwards, it is no less true, that this formula, however deceptive it may be, represents the mean results of observations sufficiently well to be employed with convenience, when an approximative quantity only is necessary: and this was exactly the case. The question was the mean temperature of mount Saint Bernard, the philosopher was Bonaparte, and the discussion took place at table, and even in the apartment of the celebrated man whose theory and calculations I was endeavouring to defend.

menſe extent of permanent ice, which covers the whole declivity of the ſummit."

But I learned, not without ſurpriſe mixed with horror, by the event, of which I am going to give an account, that this glacier, ſo often viſited by travellers, and which I twice traverſed myſelf with perfect ſecurity, contains ſome of thoſe fiſſures, covered with ſnow, which renders others dangerous when the proper precautions are not employed.

During our ſtay at Chamouni, C. d'Eymar having heard that the guide, who accompanied the unfortunate Eſchen, reſided ſomewhere in the valley, ſent for him, to learn the particulars of the accident. When he arrived, deſpair was ſtill painted in his looks and whole countenance; but we learned nothing from him that we did not before know. He was a guide, ſelected by chance, who ſeemed to be little acquainted with the mountains, and who, however, as he told us, had requeſted M. Eſchen, when they arrived at the glacier, not to ſeparate from his two companions. Hurried on, however, by that undeſcribable ſenſation which people ſometimes experience when they reach high ſummits, and obſerving at the top of the glacier, a little diſtance before him, two chamoy hunters, who were reſting themſelves, he hurried forwards to join them; and it was then that he diſappeared.

We ſhall now return to our narrative. As we paſſed Servoz on the morning of the third day, the body of the unfortunate Eſchen was conveyed thither. We viewed it with the livelieſt emotion; and by minute inſpection we were convinced that he had not ſurvived his fall a ſingle moment. We were fully confirmed in this opinion by the details which were given to us, and by obſerving that three of the real ribs on each ſide were broken, and that there was a great depreſſion of the ſternum; ſymptoms which indicate that the unfortunate young man had experienced the moſt ſudden and moſt violent compreſſion. The body was no ways diſfigured, and his features, in perfect harmony, excited no idea of his having ſuffered pain. We learned by the paſſport found in his pockets, along with other articles, that his name was Frederick Auguſtus Eſchen; that he was born

in

in the bishopric of Lubec, and that he was twenty-three years of age.

[To be concluded in the next Number.]

VIII. *Memoir on the Ibis of the Antient Egyptians.* By C. Cuvier*.

EVERY one has heard of the Ibis, a bird to which the antient Egyptians paid divine honours; images of which they placed in their temples; which they suffered to wander about unmolested in their cities; which they embalmed with as much care as they did their relations; to which they ascribed virginal purity, and an inviolable attachment to a country of which it was the emblem, and the figure of which the gods would have assumed, had they been forced to adopt one mortal.

No animal ought to have been so easy to be distinguished, for there is none of which they have left so many excellent descriptions, correct coloured figures, and even the birds themselves carefully preserved with their feathers, under the triple covering of that strong preserver bitumen, thick and close bandages, and of strong vases well covered with mastic. Yet among all the modern authors who have spoken of the ibis, there is none but Bruce, a traveller celebrated by his courage and his knowledge of natural history, who has avoided error in regard to the real species of this bird; and yet his ideas, however correct, have not been adopted by naturalists.

After several changes of opinion respecting the ibis, naturalists at present seem to agree in giving this appellation to a bird, a native of Africa, nearly of the size of the stork, with white plumage, and the wing feathers black; raised on long red legs, and armed with a long bent bill, sharp at the edges, indented at the extremity, round at its base, and of a pale yellow colour, and having its face covered with a yellow skin, destitute of feathers, and not extending beyond the eyes.

* *Journal de Physique*, Fructidor, an. 3.

Such

Such is the ibis of Perrault *, the white ibis of Briſſon †, the white Egyptian ibis of Buffon ‡, the tantalus ibis of Linnæus in the twelfth edition of his works; ſuch is the bird which in the National Muſæum is called the Egyptian ibis, and which is there placed near, and not without reaſon, to the *curicaca* of Margrave, or the *tantalus loculator* of Linnæus, for they both have a hooked, ſharp, and indented bill. To the ſame bird, according to Blumenbach, who, however, confeſſes that at preſent it is very rare, at leaſt in lower Egypt, the Egyptians paid divine honours §.

I participated in the error of theſe celebrated men above mentioned, till I had an opportunity of examining myſelf ſome mummies of the ibis. This pleaſure was afforded to me lately by Fourcroy, to whom General Grobert of the Artillery, on his return from Egypt, preſented two of theſe mummies. On unwrapping it with care, we obſerved that the bones of the embalmed bird were much ſmaller than thoſe of the tantalus; that they were ſcarcely ſo large as thoſe of the curlew; that its beak reſembled that of the latter, the length excepted, which was ſomewhat leſs, and not at all equal to the tantalus; in a word, that its plumage was white, with the wing feathers black, as mentioned by the antients.

We were therefore convinced, that the bird which the Egyptians embalmed was by no means our tantalus ibis; that it was ſmaller; and that it was neceſſary to ſearch for it among the genus of the curlew.

After ſome reſearch, we found that the mummies of the

* *Deſcription d'un ibis blanc et de deux cicognes* in the Memoirs of the Acad. of Sciences, Vol. III. p. 61. Plate XIII. *fig.* 1. The bill is repreſented as truncated at the end; but this is the fault of the draftſman.

† Numenius ſordide albo rufeſcens, capite anteriore nudo rubro; lateribus rubro purpureo et carneo colore maculatis, remigibus majoribus, nigris, rectricibus ſordide albo rufeſcentibus, roſtro in exortu dilute luteo, in extremitate auranteo, pedibus griſeis.—*Ibis candida.* Briſſon Ornith. Vol. V. p. 349.

‡ *Hiſtoire des Animaux*, Vol. VIII. 4to. p. 14. Plate I. Planches enl. No. 389.

§ Handbuch *der Natur Geſchichte*, p. 203. Edit. of 1782.

ibis, opened before our time by different naturaliſts, had been ſimilar to ours. Buffon ſays expreſsly that he examined ſeveral of them; that the birds they contained had the bill and ſize of the curlew; and yet he blindly followed Perrault, in conſidering the African tantalus as the ibis. One of the mummies opened by Buffon ſtill exiſts in the Muſæum; it is ſimilar to thoſe we have ſeen. Dr. Shaw, in the Supplement to his Travels *, deſcribes and gives an exact repreſentation of the bones of a ſimilar mummy; the bill, he ſays, was ſix inches Engliſh in length, and reſembled that of the curlew, &c. In a word, his deſcription agrees entirely with ours.

Caylus, in his *Recueil d'Antiquités* †, gives the figure of a mummy ibis, the height of which, with its bandages, was only one foot, ſeven inches, four lines; though he ſays expreſsly, that the bird was placed in it on its legs, with its head erect, and that no part was bent back in its embalmed ſtate.

Haſſelquiſt, who conſidered as the ibis a ſmall white and black heron, aſſigns as his principal reaſon for doing ſo, that the height of this bird, which is equal to that of the crow, correſponds perfectly with the height of the mummies of the ibis ‡; how then could Linnæus give the name of ibis to a bird as large as the ſtork; how, in particular, could he conſider this bird to be the ſame as the *ardea ibis* of Haſſelquiſt, which, beſides its ſmall ſize, had a ſtraight bill; and how could this laſt error, in regard to ſynonyms, be ſtill retained in the *Syſtema Naturæ?*

The only figure of the bill of an embalmed ibis, which does not accord with ours, is that given by Edwards §. It is a third larger than it ought to be; but as it contradicts all the other teſtimonies, we muſt believe that it was taken

* Edit. of Oxford, 1746. Plate V. p. 64—66.

† Vol. VI. Plate IX. *fig.* 1.

‡ Iter Paleſtinum, p. 249. Magnetudo gallinæ, ſeu cornicis; and p. 250, Vaſa quæ in ſepulchris inveniuntur, cum avibus conditis, hujus ſunt magnitudinis.

§ *Plate* 105.

from

from the mummy of ſome bird different from the ibis; or that the draftſman enlarged the proportions *.

It was neceſſary, therefore, to ſeek for the real ibis ſomewhere elſe than among theſe birds of the tantalus kind, of a great height and with a ſharp bill. In viewing the collection of birds, which Lacepede has arranged in ſuch beautiful order in the Muſæum of Natural Hiſtory, we diſcovered a ſpecies, never before mentioned or deſcribed in any of the ſyſtematic authors except Latham, and which alone correſponds to every thing indicated to us, by antient authors, monuments, and mummies, as characters of the ibis.

It is repreſented in the annexed figure (Plate III.); it is a bird of the ſize of the curlew; its bill is ſimilar to that of the curlew, but proportionally a little ſhorter, and of a black colour; two-thirds of its head from the neck are bare of feathers, and covered with ſhort black down; the plumage of the body, wings, and tail, is a dirty white, except the tips of the large wing feathers, which are black, and thoſe of the lower part of the back, which are of the ſame colour, and, being long, fall over the tips of the wings when they are folded. The feet are black, and like thoſe of the curlew.

The individual in queſtion formed part of the ſtadtholder's collection, but we are unacquainted with its native country. Deſmoulins, who has ſeen two others, aſſures us that they were both brought from Senegal; one of them was even brought from that country by Geoffroy de Villeneuve; but, beſides that the climate of Senegal is in nothing different from that of the Nile, we ſhall ſee hereafter that Bruce found this ſpecies in abundance in Egypt; and I imagine that the moderns will not admit, without ſome latitude, the aſſertion of the antients, that the ibis never quitted that ſacred land.

* Since I read this memoir, C. Olivier had the goodneſs to ſhow me the bones which he took from two mummies of the ibis, and to open with us two others. Theſe bones were ſimilar to thoſe in the mummies of General Grobert: one of the four only were ſmaller, but it was eaſily obſerved by the epiphyſes that they had belonged to a young individual. C. Olivier ſhowed us alſo a bill two-thirds longer than thoſe found commonly in mummies of the ibis; but this bill, however, was perfectly ſimilar to that of the curlew; and in particular to that of the black ibis of Belon; but in no manner to that of the tantalus.

Beſides,

Beſides, this aſſertion would be as much contrary to the tantalus ibis as to our curlew; for the individuals which we poſſeſs of that ſpecies, have been brought from Senegal. It was from this country that Geoffroy de Villeneuve brought the one in the Muſæum of Natural Hiſtory: it is even much rarer in Egypt than our curlew, ſince, according to Perrault, no one had ever ſaid that he ſaw it there, or had received it from that country.

Since we are now acquainted with this bird, if we examine the works of the antients, and antient monuments, we ſhall find every difficulty vaniſh, and that all their teſtimonies agree with the beſt of all, which is, the body of the bird itſelf preſerved and embalmed. "The moſt common kind of the ibis," ſays Herodotus *, "has the head and neck bare, the plumage white, except the head, the neck, the tips of the wings, and the rump, which are black. The bill and feet reſemble thoſe of the other kind of ibis:" and he had ſaid of the latter, "they are all black, have feet like the crane, and a hooked bill." There are many modern travellers who do not give ſuch good deſcriptions of the birds they obſerve as that which Herodotus has given of the ibis. How could naturaliſts apply this deſcription to a bird having no part naked but the face, which is red? To a bird which has the rump white, and not black? This laſt character, however, was eſſential to the ibis. Plutarch ſays, in his Treatiſe *De Iſide et Oſiride*, that the manner in which the white was mixed with the black in the plumage of this bird, made it appear as if marked with a creſcent; and, indeed, the union of the black of the rump with that of the two tips of the wings, which forms on the white a large ſemicircular indentation, makes the white have a reſemblance to that figure.

It is more difficult to explain what he means when he ſays that the feet of the ibis form with its bill an equilateral triangle.

The paintings of Herculaneum †, however, place the matter beyond all doubt. Some of theſe paintings, which repreſent Egyptian ceremonies, exhibit ſeveral of theſe birds

* Euterpe.

† Plates, No. 138 and 140 of the edition of David; and Vol. II. p. 335, No. 59; and p. 321, No. 60, of the original edition.

walking in the courts of the temples: they are perfectly ſimilar to the bird which we have pointed out: we may in particular obſerve in them the characteriſtic blackneſs of the head and neck; and it may be readily ſeen, by comparing their ſize with that of the perſonages in the paintings, that it muſt have been a bird of about half a yard in height at moſt, and not a yard, like the tantalus ibis.

The moſaic of Paleſtrine exhibits alſo in its middle part ſeveral ibiſes perched on buildings, and which are in nothing different from thoſe of the paintings of Herculaneum. A ſardonyx in the cabinet of Dr. Mead, copied by Dr. Shaw *, and repreſenting an ibis, ſeems to be a miniature of the bird which we are deſcribing. A large bronze medal of Adrian, an engraving of which is given in the Muſæum de Farneſe, and another of the ſame emperor of ſilver †, exhibit figures of the ibis, which, notwithſtanding their ſmall ſize, have a great reſemblance to the bird in queſtion.

In regard to the figures ſculptured on the plinth of the ſtatue of the Nile at the Belvedere, and in the copy of it in the garden of the Thuilleries, they are not well enough finiſhed to ſerve as proofs; but we ſtill have a ſufficient number to obviate every doubt.

We muſt do Bruce the juſtice to ſay that he diſcovered the real ibis ‡. His *abou-hannès*, compared with the bird we have deſcribed, is ſo ſimilar, that it is hardly poſſible not to conſider it as of the ſame ſpecies: and this traveller ſays expreſsly, that it ſeemed to him to reſemble thoſe contained in the repoſitories of the mummies. He ſays alſo, that this *abou-hannès*, or Father John, is very common on the banks of the Nile; while the bird repreſented by Buffon under the name of the white Egyptian ibis, was never ſeen there by him. This *abou-hannès* has been inſerted by Latham, in his *Index Ornithologicus*, under the name of Tantalus Æthiopicus; but he does not ſpeak of the conjecture of Bruce reſpecting its identity with the ibis.

* Appendix, Plate V.

† Muſæum de Farneſe, Vol. VI. Plate XXVIII. fig. 6.; and Vol. III. Plate VI. fig. 11.

‡ Vol. V. p. 172, Engliſh edition.

All

All travellers who preceded or followed Bruce, ſeem to have been in an error. Belon * gave the name of black Ibis to a bird, which is nothing elſe than a black curlew with a naked head, and the feet and bill red; which does not agree with the deſcription of Herodotus, who ſays that the black ibis is black all over.

This bird of Belon is very common in collections; and yet as naturaliſts ſought alſo in the black ibis a tantalus with a ſharp bill, the modern naturaliſts have almoſt all ſaid that Belon alone had ſeen this bird. Lacepede has rectified this error, and given the name of black ibis to the bird which had been diſtinguiſhed by that name by Belon. In regard to the white ibis, Belon believed it was the ſtork; in which it is evident he contradicted every teſtimony: no one, therefore, has coincided in opinion with him in this point except the apothecaries, who have taken the ſtork for their emblem, becauſe they have confounded it with the ibis, to which they aſcribe the invention of injections.

Proſper Alpinus, who aſcribes this invention to the ibis, gives no deſcription of the bird in his work on the Medical Art of the Egyptians †. In his Natural Hiſtory of Egypt ‡ he ſpeaks only from the information of Herodotus, to whoſe account he only adds, I know not from what authority, that this bird in its figure and ſize reſembles the ſtork. He ſays he had learned that both the black and the white kinds were found in abundance on the banks of the Nile; but it is evident that he did not believe that he had ever ſeen any of them.

Shaw, ſpeaking of the ibis, ſays § that at preſent it is exceedingly rare, and that he had never ſeen it. His emſeeſſy, or ox-bird, which Gmelin, very improperly, refers to the tantalus ibis, is of the ſize of the curlew, has a white body, and the feet and bill red. It frequents the meadows near cattle; its fleſh is not well taſted, and ſoon ſpoils. It may be eaſily ſeen that this is not the tantalus, and ſtill leſs the ibis, of the antients.

* De la Nature des Oiſeaux, Book IV. chap. 9, of the edition of 1555.
† De Medicina Ægyptiorum, Lib. I. fol. 1. Paris, 1646.
‡ Rerum Egypt. Lib. IV. cap. 1. Leyden, 1735.
§ Page 255, Emſeeſſy ox-bird

Haſſelquiſt was acquainted neither with the white nor the black ibis: his ardea ibis, is a ſmall heron with a ſtraight bill. Linnæus, in his tenth edition, placed it, very properly, among the herons; but he was wrong, as I have already ſaid, in tranſporting it as a ſynonym of the genus of the tantalus.

Maillet, in his Deſcription of Egypt*, conjectures that the ibis may have been a bird peculiar to Egypt, which is called there Pharaoh's capon, and at Aleppo, *ſaphanbacha*. It devours ſerpents. Some of theſe birds are white, and ſome black and white, and it follows, more than a hundred leagues, the caravans which go from Cairo to Mecca, in order that it may feed on the carcaſes of the animals killed in the courſe of the journey, while in every other ſeaſon none of them are ſeen on that route. But he does not conſider this conjecture as certain. He even ſays that we muſt give over attempting to underſtand the antients when they ſpeak in ſuch a manner as if they had wiſhed not to be underſtood; and he concludes with obſerving that the antients perhaps comprehended, indiſcriminately, under the name of *ibis*, all the birds which were ſerviceable to Egypt in freeing it from thoſe dangerous reptiles which the climate produces in abundance, ſuch as the vulture, falcon, ſtork, kite, &c.

He was right in not conſidering his Pharaoh's capon as the ibis; for, though his deſcription be very imperfect, and though Buffon thought he could diſtinguiſh in it the ibis, it may be readily ſeen, as well as by what Pococke ſays, that this bird muſt be carnivorous; and, indeed, we find by the figure which Bruce has given †, that Pharaoh's capon is nothing elſe than the *rachama*, or ſmall white vulture with black wings, *vultur percnopterus*, Linn., a bird exceedingly different from that we have proved to be the ibis.

Pococke ſays that it appears, by the deſcriptions given of the ibis, and by the figures of it found in the temples of Upper Egypt, that it is a kind of crane. "I have ſeen," adds he, "a number of theſe birds in the iſlands of the Nile; they were, for the moſt part, grayiſh ‡." Theſe few

* Part II. p. 23. † Vol. V. p. 101.

‡ French tranſlation of his Travels, 12mo. Vol. II. p. 53.

words

words are ſufficient to prove that he was no better acquainted with the ibis than others.

The error which prevails at preſent in regard to the white ibis, began with Perrault; who is even the firſt perſon who deſcribed the tantalus ibis of the preſent time. This error, adopted by Briſſon and Buffon, has paſſed into the twelfth edition of Linnæus, where it is confounded with that of Haſſelquiſt, which had been inſerted in the tenth, to form with it a compound altogether monſtrous. It was founded on the very natural idea, that to devour ſerpents, a ſharp bill, more or leſs analogous to that of the ſtork or heron, was neceſſary. This idea is even the only good objection that can be made againſt the identity of our bird and the ibis; for it may be ſaid, How can a bird with a weak bill, ſuch as the curlew, be able to devour theſe dangerous ſerpents?

But, beſides that a reaſon of this kind cannot hold out againſt poſitive proofs, ſuch as deſcriptions, figures, and mummies; beſides that the ſerpents from which the ibis delivered Egypt were accounted exceedingly venomous, but not as of a large ſize; I could anſwer directly, that the mummified birds, which had a bill abſolutely ſimilar to that of our bird, were real devourers of ſerpents; for I found in one of their mummies the undigeſted remains of the ſkin and ſcales of ſerpents, which I preſented to the Claſs. This deſtroys the objection that might be drawn from a paſſage of Cicero, where he gives to the ibis a corneous and ſtrong bill: having never been in Egypt, he imagined, from ſimple analogy, that this muſt be really the caſe. Our European curlew, which has a bill ſtill weaker than the ibis, devours eels, as I have been aſſured by an eye-witneſs.

I ſhall conclude this memoir with announcing the reſults. The tantalus ibis of Linnæus muſt remain a ſeparate genus with the *tantalus locuator*. Their character will be *roſtrum validum arcuatum, apice emarginatum.*

The other tantalus of the laſt editions ought to form a genus with the common curlew. We may give them the name of *numenius*. Their character will be, *roſtrum gracile, arcuatum, apice inflatum.*

The ibis of the antients is not the ibis of Perrault and Buffon,

Buffon, which is a tantalus; nor the ibis of Haſſelquiſt, which is an ardea; nor that of Maillet, which is a vulture; but a *numenius* or curlew, which has not yet been named by ſyſtematic authors, and of which Bruce has given a figure under the name of the *abou-hannès*. I call it *numenius ibis, albus, capite et collo nudis, remigibus pennis uropygii elongatis, roſtro et pedibus nigris.*

The tantalus ibis of Linnæus, in the preſent ſtate of his ſynonymy, comprehends four ſpecies, of three different genera:

A tantalus, the ibis of Perrault and Buffon;
An ardea, the ibis of Haſſelquiſt;
Two numenii, the ibis of Belon;
And the ox-bird of Shaw.

The reader may judge by this inſtance, and many others, of the ſtate in which the *Syſtema Naturæ* ſtill remains, and of what importance it would be to free it gradually from thoſe errors with which it abounds, and with which naturaliſts ſeem to load it ſtill more, by accumulating, without choice and proper examination, ſpecies, characters, and ſynonyms.

IX. *A brief Examination of the received Doctrines reſpecting Heat or Caloric. By* Alexander Tilloch. *Read before the Aſkeſian Society, December* 1799 *.

To detail the various theories maintained at different periods, and applied to explain the phenomena to which heat gives riſe in the numerous changes preſented by Nature in chemical combinations and decompoſitions, would be taking up the time of this Society unneceſſarily, as every member muſt be well acquainted with them. I ſhall therefore confine myſelf to the modern doctrines on this ſubject; and even with theſe I ſhall be brief, as my chief object is, to inquire, Whether none of the circumſtances that accompany the known facts in this department of phyſics (ſuch of them, I mean, as the limits I have preſcribed to the preſent eſſay will

* For a ſhort account of this Society, ſee Note, p. 353, Vol. VII.

allow me to notice) have been overlooked? and whether, If all of them had been attended to, and the legitimate inferences been drawn from them, the theory now generally received would ever have been admitted into ſcience?

The ground I am about to take may perhaps expoſe me to the danger of being conſidered as an innovator, or, what may be deemed worſe by ſome, a ſceptic as to certain opinions, rendered reſpectable by the great names that have embraced and maintained them; and at the ſame time extremely plauſible from the apparent facility with which they are applied to explain many phenomena that daily preſent themſelves to the eye of every attentive obſerver. My only anſwer is, that I am in ſearch of truth; and ſo decided an enemy to mere theoretical ſpeculations, that I neither admit myſelf, nor wiſh others to admit in phyſics what cannot be proved to be truth. When the human mind acquieſces on any ground ſhort of this, it is either through miſconception, indolence, or puſillanimity, than which nothing has tended more to retard ſcience and ſhackle men with prejudices, leading them to receive great names for argument, and, for demonſtration, long quotations. But, to proceed, I uſe the term *heat* to denote that ſubſtance which poſſeſſes thoſe properties, is governed by thoſe laws, and produces thoſe effects which ſhall be immediately enumerated; and I prefer it to the term *caloric** for no other reaſon but becauſe the latter is employed by many to denote heat exiſting in a certain ſtate, in which, they ſay, it may be conſidered as having actually loſt its original character: whereas I hold, and I even hope to convince the members of this Society, that it invariably retains the ſame character, properties, and mode of action.

* The term *caloric* has been adopted in the new nomenclature to avoid that ambiguity and miſconception which might, it is ſaid, ariſe from employing the ſame term to expreſs a ſubſtance, and the ſenſation produced by the action of that ſubſtance. By the ſame mode of reaſoning, all the *ſubſtantives* ſhould be changed in any language that has ſimilar *verbs*. But ſuppoſe the argument for change, in the preſent inſtance, to have full force in ſome languages, it has little or none in regard to the Engliſh, which employs the word *warmth* to expreſs the ſenſation occaſioned by *heat*. I ſhall, however, uſe the terms heat and caloric indifferently.

(A) Heat is diffuſed through all the bodies in nature; whether ſolids, liquids, or aëriform fluids.

(B) Heat tends to an equilibrium; ſo that, when by any means it is accumulated in particular ſubſtances, a portion is quickly given off to the ſurrounding bodies to bring the whole to one common temperature. On the other hand, where bodies have been deprived of a portion of it, heat is given off to them by, or heat paſſes to them from, the ſurrounding bodies, to reſtore the equilibrium.

(C) Solids, by the addition of heat, aſſume the form of liquids, and liquids the aëriform ſtate. On the other hand, gaſes, by an abſtraction of heat, become liquids; and liquids, ſolids.

(D) The dimenſions of bodies are enlarged when heat is poured into them, and *vice verſa*.

The laws we have enumerated are general, and the objections that may be ſtated againſt the truth of any of them ſo few, and ſo eaſily obviated, that they cannot affect any inference drawn from them. The apparent exceptions relate chiefly to the change of volume in ſome particular ſubſtances when paſſing from the liquid to the ſolid form, or the contrary. Water, for inſtance, in paſſing into the ſtate of ice, aſſumes a larger volume, though heat is then paſſing out of it. The ſubſtance, however, is only apparently enlarged. In freezing, the water aſſumes a cryſtalline form; the cryſtals, ſhooting in every direction, crowd againſt, and, as it were, joſtle each other, cauſing vacuities, which conſtitute no real part of the matter. We may compare this phenomenon to what takes place in a bundle of chips of wood, which will always, however cloſely packed, occupy an apparently larger ſpace than the ſame weight of ligneous matter as arranged by nature in the tree. Another apparent exception may be noticed in clay, which diminiſhes the more in bulk the greater the quantity of heat poured into it: but here there is a miſnomer—it is not clay, but *a mixture of clay and water* that is diminiſhed in bulk. The water is driven off; and, where there is a diminution of matter, a reduction of volume muſt follow. Clay is therefore not an exception to

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the general law—drive all the water out of it, that is, convert it into glaſs, and it then obeys the general law.

We ſhall firſt attend a little to the operation of the foregoing laws in ſome given caſes; and afterwards examine the doctrines which have been eſtabliſhed from the phenomena obſerved to accompany them:

If two bodies of the ſame nature, unequally heated, be brought into contact, the heat will diffuſe itſelf equally through them, and the quantities in each will bear the ſame proportion to each other as the maſſes themſelves; but, if the bodies differ in kind, though equilibrium take place, and each indicate the ſame temperature, yet the proportions of heat in each will not be as the maſſes, but vary according as the bodies differ.

(E) This property of bodies to hold different quantities of heat, even when that fluid is in a ſtate of equilibrium, Dr. Black calls *the capacity of a body for heat*; the quantity itſelf he calls *ſpecific heat*. The diſtinction is perfectly philoſophical, nor need the terms be objected to; but they ſhould be accurately defined, that they may never be employed to convey ideas different from the facts that gave riſe to their adoption.

That the ſenſe in which we underſtand and uſe the terms may be clearly conceived, we ſhall ſubſtitute another ſubſtance for heat, all other circumſtances being as before. If two ſubſtances of the ſame kind (two pieces of chalk), unequally wetted with water, be brought into contact, the one will give off water to the other (or the one will abſorb water from the other) till the quantity in each be *as their maſſes*. If two bodies different in kind (chalk and wood) be thrown into water, the quantity they will each take in will be *as their capacities*: this will alſo be the caſe if, inſtead of throwing them into water, we ſuſpend both in the ſame damp atmoſphere; and the quantity in each, whether received by immerſion in water or in the atmoſphere, is *ſpecific*. The latter term muſt be thus conſidered, otherwiſe we ſhall lead ourſelves into error; for there is at leaſt as great a difference between the extremes of the general temperature in ſummer and in winter as there is between a moiſt, and a comparatively dry (but yet moiſt) atmoſphere.

Some philoſophers tell us that *the cauſe* of this phenomenon of different bodies requiring different quantities of caloric to indicate the ſame temperature (or, in other words, the cauſe of their having different capacities for heat) ariſes from the different degrees of affinity which different bodies poſſeſs for heat! Is this an explanation?—Different bodies hold different quantities of heat, *becauſe they have different capacities*:—Different bodies hold different quantities of heat, *becauſe they have different affinities*!—yet we are to conſider the one term as expreſſive of a *property*, and the other as expreſſive of *the cauſe* of that property! Is this conſiſtent with that accuracy and preciſion which ſhould prevail in the language of ſcience?

But this is not all. Heat is conſidered as exiſting in two diſtinct ſtates; free, and in chemical union. When heat is in equilibrio in any place; in other words, when a body is in equilibrio with the bodies which ſurround it with reſpect to its heat [that is, has received heat from the common ſtock proportioned to its capacity]; that quantity which it contains is termed *latent heat* or *caloric*; *combined caloric*; *heat in chemical union*. We are told that in this ſtate it is not perceptible by any external ſign or organ of ſenſe; that it does not affect the thermometer, but remains quieſcent in thoſe bodies of which it conſtitutes a principle; and that it is then, more or leſs, in a ſtate of confinement.

Is the heat in any body termed *latent* becauſe the body has not a capacity to receive more? No; for if, by any means, an addition be made to the common ſtock of a preſcribed ſyſtem of bodies, each individual ſubſtance will ſtill provide lodging for a ſhare of the added quantity, proportioned to its capacity, in relation to the capacity of the reſt. It ſhould be obſerved too, that, let the common temperature be what it may, if heat be in equilibrio, the portion in each body is, by ſome at leaſt, held to be *latent*, in contradiſtinction to what they term *free heat*. Hence it follows, that the quantity of *latent heat* in a given body differs in different ſeaſons of the year; for heat may be in equilibrio in any ſyſtem of bodies in ſummer as well as in winter. An aſſemblage of different bodies, in equilibrio as to caloric, in

Jamaica,

Jamaica, the quantity in the whole is *latent*, and in each *specific* and *latent*: the same holds with regard to a system of substances in England in the month of January. The heat in both cases is *latent*. If the one system of bodies could be transported to the other in a moment, and placed in contact with it, a portion of that heat which was *latent* in Jamaica, would be counted *sensible* heat in London! This is exactly where the doctrine lands us when viewed generally; and yet we are to consider heat as having two distinct modes of existence! But let us take a closer view of it:

When by the passing of a solid body into a fluid form the surrounding atmosphere is found to have lost heat, this heat, they say, has not been merely absorbed by the substance that has become fluid, but has become latent in it. When a liquid passes into the aëriform state, the same phenomenon accompanies the change, and also the same assertion; and the doctrine is meant to convey the idea that heat has not only been changing its place, but has itself undergone a change as to its state, its properties, and mode of action.

It is a generally received axiom, that no more causes should be admitted in physics than what are true and sufficient to account for the phenomena. Let us inquire whether this axiom be not violated in the cases stated, when recourse is had to the doctrine of latent, as distinguished from free, heat, to assist in explaining the phenomena.

The dimensions of bodies are enlarged when heat is poured into them (D). The primitive moleculæ (independent of the heat) of which the bodies are formed, are forced farther from each other by the interposed matter of heat. Continue the action of the same cause, their cohesion will be destroyed entirely (C), and they will be diffused through the predominating substance (heat), as the particles of saline substances diffuse themselves through water when dissolved in that liquid. In the case just put for illustration, whoever supposes the water to have undergone any change as to its essence or properties, whether directly poured upon the saline substance or furnished to it by the atmosphere.

In chemical combinations it is admitted that we cannot,

a priori, determine the properties of the compound from a previous knowledge of thoſe of the ingredients. Every ſubſtance has, for one of its properties, *a capacity for heat*. If two be united chemically, the capacity of the compound for heat may not be that of the ſum of the capacities of the ingredients for the ſame ſubſtance. Indeed it ſeldom or never turns out ſo. If by any proceſs then we change the capacity of two or more bodies; or, by uniting them, produce a third, whoſe capacity for heat is greater than the ſum of that of the ingredients, this new compound muſt demand from the common ſtock of heat diffuſed through the atmoſphere and other neighbouring ſubſtances (A) an additional portion. Is there any thing in all this to lead us to the notion that the paſſage of heat from the atmoſphere, or other contiguous bodies, into the new compound, is the operation of any other than the general law by which heat tends to an equilibrium among all the bodies in a ſyſtem (B), and diffuſes itſelf among them in proportion to their capacities for heat (E)? Is not the operation of theſe laws ſufficient to explain the phenomena without the aid of any others?

But, beſides, is it not a general fact, in all caſes where a capacity for a greater quantity of heat is produced in a compound than the ſum of the capacities of the ingredients, that the ſum of the volume of the compound is greater than that of the ingredients? And is it not equally true, that, when ingredients, before in union, are found, on being ſeparated, to have acquired capacities for heat, whoſe ſum is greater than was that of the body in which the ingredients were united, the ſum of the volume of the ingredients is greater than the volume of the compound was *? Can it be correct to ſay that it neither affects our ſenſes, nor is cogniſable by any external ſign! when we can ſee with our eyes its effect in the increaſe it has made in the volume of

* At the moment of writing I do not recollect a ſingle exception. In caſes where cryſtalliſation is concerned there may be ſome apparent one, as in the caſe of water and ice; but theſe will not affect my argument. It is enough for me that *the effect* holds generally in the caſes to which the doctrine of latent heat is applied—*an effect* perfectly conformable to the general law (D), and alſo *cogniſable by our ſenſes*.

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the compound substance before us; or in the sum of the masses of two or more substances over that of the compound from which they were separated?

Again—When gases pass into the form of liquids, or liquids into the solid state, heat is found to pass from them into the atmosphere, "the heat that was latent in them before now becomes sensible." Was it *not cognisable* before? It ought not to have been so when latent. I find, however, that the volume of the body from which it has been separated is now *less* than before; and this is exactly what ought to take place from the general laws, not only of heat but of matter—a substance, heat, has been separated from a body in which it formed a part of the mass or volume, and the remaining mass is diminished in bulk.

Let us, however, alter the mode of expression. A body, no matter by what means, is reduced into a less volume; it is so constituted as to admit of this effect being passed upon it; for one of its ingredients, heat, is a subtile fluid which may be dislodged; nay, will even run out, when the capacity of the body which held it is lessened. It is no way strange, in this case, that a fluid poured out of its recipient, and which, by its tendency to equilibrium, must diffuse itself through the surrounding bodies in proportion to their capacities to receive it, should make itself manifest to our sense of feeling, we being immersed at the moment in one of the neighbouring substances, the atmosphere, which affords a lodging for a portion of the dislodged heat, and serves to transport other portions of it to all the neighbouring bodies. Nor is it strange that a substance which, while it formed an ingredient in the body from which it has been removed, constituted also a part of its volume, should, when it passes into another body, mercury in a glass tube, cause an increase in its volume proportioned to the quantity that has passed in.

It constituted *sensible bulk* in the first body, and was *latent heat*; it constitutes *sensible bulk* in the second body also, but there it is *sensible heat!!* Is there no absurdity in all this?

Heat in those combinations, in which the term *latent* is employed, not cognisable by our senses! Is it not obvious from

from the preceding ſtatement that this is a miſtake? Before the heat paſſed out of its then combination it made a part of the volume which we contemplated with our eyes, and the proportion that this part bore to the whole might, in many caſes, be determined by actual meaſurement. Is ſenſible heat, or rather heat in ſuch combinations as it is in when that term is uſed, cogniſable by any other means?

To me it would appear as correct, when, by any power, the capacity of a veſſel filled with a liquid (water) is diminiſhed, to ſay that the portion of liquid thereby ejected is ſet at liberty; meaning thereby that it has undergone an eſſential change as to its form, properties, or mode of exiſtence; as to ſay, when the volume of a ſubſtance containing heat is abridged, and a proportionate quantity of the heat is diſlodged, that the heat, ſo diſlodged, has undergone any ſuch change.

The fact in my opinion is, the ſubſtance in which the caloric reſided has undergone the change, where there is a change; not the heat: or, if it be changed, the change is of that kind which paſſes upon a brick or any other body when we move it from one room to another. This, I think, muſt be pretty clear from what we have already ſtated; but I hope to make it ſtill more ſo by examining a particular caſe or two which have been made to ſerve for a foundation, as it were, for the doctrine which I oppoſe.

[To be continued.]

NEW PUBLICATIONS.

An Account of the Irides or Coronæ which appear around, and contiguous to, the Bodies of the Sun, Moon, and other Luminous Objects. Cadell and Davies, 1799. 46 pages. With One Copperplate.

THIS treatiſe is by the author of the *Obſervations on the Inflexions of Light*, of which a ſhort account was given in the Philoſophical Magazine for September. In making theſe obſervations, that ingenious gentleman perceived that a ray

a ray of white light, paſſing between angular ſurfaces, ſuch as the almoſt meeting edges of two knives, was liable to be parted by its bendings, ſeparations, and other changes, into thoſe differently coloured rays, which had been ſuppoſed by Newton to be the primogenial elements of pure white light. This obſervation is here applied to explain the formation of thoſe *irides or coronæ, conſiſting of many-coloured circles*, which are ſeen contiguous to the bodies of the ſun, moon, &c.

The order of the colours in theſe coronæ is as follows: 1. Next to the body of the luminary a circle of grayiſh black, grayiſh blue, or faint diluted white, altering as it recedes into—2. A ſtrong lucid white of conſiderable breadth. 3. Slender rings of yellow and red. 4. A ſucceſſion of rings of violet, blue, green, yellow, red. 5. Green, diluted yellow, red; diluted green, diluted red. This is the common order; but it is occaſionally varied by ſlight irregularities.

Newton has made the only plauſible attempt to account for theſe appearances. He ſuppoſes them to be produced by the rays of light falling upon globules of water, hail, or ice. He aſſumes that globules of ice or hail may, or actually do, exiſt in ſituations in the atmoſphere in which they can receive the rays, ſo as to produce that analyſis of the white light, and that diſtribution of the coloured rays which theſe coronæ exhibit. He conjectures that theſe coronæ in the atmoſphere, ſurrounding the ſun or moon, may be produced in the ſame manner in which a miniature appearance ſomewhat ſimilar is occaſioned by the reflexion of light from a lens quickſilvered on the back-ſide.

But we do not know particles of hail or ice always to exiſt in the atmoſphere when theſe many-coloured irides or coronæ appear. The ordinary refraction of direct light, in its paſſage through globules of liquid water, could not produce theſe appearances. Newton's theory, though all that he poſtulates were granted, would not explain that particular diſtribution of theſe colours in the coronæ, by which intervals or vacant ſpaces are left between the different portions of the red and of the other colours. The iris, from a globule of cryſtal having quickſilver on its further ſide, is, according to Newton, produced, not by the reflexion of the main beam of light,

light, but by a *faint light* irregularly ſcattered in every direction from that beam: and a light ſo *faint* would ſcarcely be equal to the production of the many-coloured irides round the ſun, moon, &c. in the circumſtances in which theſe appear; nor, indeed, could the globules of water fall under any light ſo faint, where the main beam to which they are expoſed is broader than their whole ſurfaces.

On the other hand, the author of this eſſay has obſerved that the *arrangement* of the colours and orders of colours in the many-coloured irides of which he treats, is not only impoſſible by any refraction that can be ſuppoſed; but *is perfectly the ſame with the arrangement of the colours in the two ſets of fringes into which a ray of white light, admitted into a dark chamber, and made to paſs between two parallel edges, is naturally divided.* Each of theſe two ſets of fringes, if taken outward from the central line, exhibits preciſely the ſame arrangement of colours, as does the ſolar iris taken from its interior to its exterior extremity. He therefore concludes that, in the formation of the many-coloured irides, *the ſun's irradiation, paſſing between the edges of contiguous drops of water in the atmoſphere, is, by the inflecting attraction which theſe exerciſe upon it, ſeparated into two ſets of fringes, which fringes conſtitute the irides, having the body of the ſun for their central line.* This is the author's theory.

It is at leaſt admirably ingenious; more ſimple and plauſible than that which it ſtrives to ſuperſede; and founded upon a better induction of unqueſtionable facts. We ſhould hope that the general diſcuſſion of philoſophers muſt finally eſtabliſh it as ſcientific truth. The eſſay is elegantly and unaffectedly written.

A Treatiſe on the Chemical Hiſtory and Medical Powers of ſome of the moſt celebrated Mineral Waters, &c. By William Saunders, *M.D. F.R.S., &c.* Phillips, George-yard, Lombard-ſtreet. 8vo. 483 Pages.

THE more accurate analyſis of mineral waters by modern chemiſtry, cannot but lead national phyſicians to eſtimate more juſtly than before, the nature of their reſpective medicinal powers.

In

In the work now before us, Dr. Saunders applies, with conſiderable ſkill, the information of chemical analyſis to aſſiſt in explaining the proper remedial uſes of the waters of the moſt celebrated mineral ſprings in Britain and on the Continent.

His work begins with an account of the phyſical qualities and the chemical compoſition of water, and of its natural uſes in the animal economy. He then examines, what extraneous ſubſtances are liable to exiſt in ſolution in water, and by what chemical teſts or re-agents the preſence of any of them may be aſcertained. A judicious chapter follows next, concerning the nature, as more or leſs ſalutary, of the waters in common uſe.

After this preliminary matter he deſcends into a particular account of the chemical analyſis and the medical uſes of three-and-twenty different ſpecies of mineraliſed waters. Theſe are, the waters of Malvern, Holywell, Briſtol Hotwell, Matlock, Buxton, Bath, the Sea, Sedlitz, Epſom, Seltzer, Tunbridge, Spa, Pyrmont, Cheltenham, Scarborough, Vichy, Carlſbad, Hartfell, Harrogate, Moffat, Aix-la-Chapelle, Borſet, Barege. A ſynoptical table, exhibiting, in one view, the chemical compoſitions of all theſe different waters, is ſubjoined at the cloſe of the chapter.

The medical uſes of water, are, next, *more particularly* conſidered. Water in general, as he here ſtates, is uſeful, in proper quantity, to dilute and ſuſpend the ſolids which are taken into the ſtomach, ſometimes to ſtimulate languid digeſtion; ſometimes, perhaps, to retard that which might be otherwiſe too rapid. In acute diſeaſes, its internal uſe as a *diluent* is highly beneficial. Tepid water, taken in moderate doſes, acts, generally, as an agreeable ſtimulant to the ſtomach. A draught of water, at the greateſt warmth at which it can be drunk, will commonly give ſudden relief from *heart-burn*. To ſedentary perſons, in acute fevers, for topical inflammations, cold water and the cold bath may be very uſefully employed, and in ice for burns. The *warm*, or *tepid* bath, is adapted to prove ſerviceable in, perhaps, a ſtill greater variety of inſtances. Artificial chemical waters are not without their uſe.

Whether medically or chemically conſidered, this work is, in great part, a compilation. It does not appear that Dr. Saunders has himſelf actually analyſed the different mineral waters of which he here gives the hiſtory: but he may have had opportunity to obſerve the effects of them all, when adminiſtered as remedies. His compilation is made with diligence, with diſcrimination, and with evident care for accuracy. A rich vein of medical good ſenſe runs through the whole work; and it is even much that the authority of a phyſician ſo truly reſpectable as Dr. Saunders, is added to that of the different medical writers, by which the remedial uſes of theſe waters had been before examined and explained.

General Zoology, or Syſtematic Natural Hiſtory. By GEORGE SHAW, *M.D. F.R.S. With Plates from the firſt Authorities and moſt ſelect Specimens.* Vol. I. Mammalia. Kearſley, London; 1800. 8vo: 552 Pages, 121 Plates.

AFTER the ſtudy of man, what can be more intereſting or uſeful than to examine the different forms, colours, and manners of the lower animals; to trace their local hiſtory over the earth, as they are in their reſpective ſituations to be met with; to acquaint ourſelves with their relations to the uſes of human life, as well as with the mutual connections and dependencies of their various ſpecies on the reſt of nature, and on one another?

We owe to two illuſtrious foreign naturaliſts, De Buffon and Linnæus, thoſe ſyſtems of natural hiſtory by which the knowledge of this ſcience has been, in the courſe of the preſent century, the moſt ſucceſsfully diffuſed. De Buffon, writing in a popular language, and with great ſplendour of ſtyle, has become the natural hiſtorian of the people: Linnæus, ſeeking his chief merit in the excellencies of ſcientific enumeration and arrangement, has become the guide of philoſophers. Buffon is quoted as good authority: Linnæus is followed, with implicit reverence, as a maſter. The ſchool of Buffon is now, almoſt excluſively, confined to his continuators and editors: all Europe has adopted the arrangement and the terms of Linnæus; and his influence is as great in natural

natural hiſtory, as is that of Sir Iſaac Newton in natural philoſophy, or that of Lavoiſier in chemiſtry.

In this volume Dr. Shaw gives the firſt part of a general ſyſtem of zoology, in which the arrangement of Linnæus is to be chiefly followed. Quadrupeds, birds, amphibia, fiſhes, inſects, vermes, teſtaceous animals, zoophytes, are to be comprehended in the ſyſtem. It is to be completed in ten or twelve volumes. The preſent volume comprehends the hiſtory of the primates, bruta, and feræ, the three firſt orders in the claſs of mammalia or viviparous quadrupeds. The peculiar advantages of this ſyſtem are, as the author hopes, to ariſe from the combination of the arrangement of Linnæus with the materials contained in the works of Buffon and Pennant; from the correction of errors relative to ſynonyms; the inſtitution of new ſpecies; the addition of more clearly diſtinctive ſpecific characters; from the communication of thoſe new facts in natural hiſtory which have been learned in late voyages to the South Sea, and particularly by obſervations made in the great iſland of Auſtralaſia, or New Holland. The deſcriptions are illuſtrated, as the title-page expreſſes, with engravings of the forms of many of the animals deſcribed. Theſe engravings are copied from the works of Buffon and other naturaliſts, from various original drawings, and from ſpecimens in the Leverian and the Britiſh muſæum. The hiſtory of thirty-eight different genera of quadrupeds is contained in the firſt volume. The engravings are, in general, well executed by Heath and other artiſts.

It is extremely probable that the whole work, if conducted to an end with the ſame care and ſkill which appear in the firſt volume, will preſent a fuller and more correct ſyſtem than has been as yet publiſhed of our knowledge in zoology. Its chief *merits* are, great clearneſs and accuracy of deſcription, fidelity of repreſentation in the engravings, eaſe and perſpicuity of ſtyle, juſt diſcrimination as to diſtinctive characters, and the increaſe which it affords of the number of known animals. Its chief *imperfections* are, that the author appears to have ſtudied more in cabinets of curioſities, than by converſing with living nature; that he aſſumes for his autho-

rities in facts, rather the writers of former ſyſtems, than the narratives of original obſervations from which thoſe writers compiled; that, while intent chiefly upon accurate deſcription, he has told little, except in a few popular inſtances, of the manners, habits, and local relations of the quadrupeds which he deſcribes.

The Aſiatic Reſearches; or Tranſactions of the Society inſtituted in Bengal for inquiring into the Hiſtory and Antiquities, the Arts, Sciences, and Literature of Aſia. Vol. VI. Calcutta, 1800.

We are happy to find that this Society ſtill continues its labours with unabated ſpirit and induſtry. The contents of this volume are:

1. A Diſcourſe delivered by Sir Robert Chambers, Knt. Preſident.—2. Narrative of a Journey from Agra to Oujein.—3. An Account of the Inhabitants of the Poggy or Naſſau Iſlands, lying off Sumatra.—4. Obſervations on the Theory of Walls, wherein ſome Particulars are inveſtigated which have not been conſidered by Writers on Fortification.—5. On the Poiſon of Serpents: Supplement to the foregoing Paper.—6. An Account of Petroleum Wells in the Burmha Country.—7. On the Maximum of Mechanic Powers, and the Effects of Machines when in Motion.—8. On the Religion and Literature of the Burmas.—9. Narrative of a Journey to Sirinagur. Enumeration of Plants noticed in the preceding Tour. Letter from Sir C. W. Malet, Bart. to the Preſident, on the Subject of the following Paper:—10. Deſcription of the Caves or Excavations on the Mountain about a mile to the Eaſtward of the Town of Ellora.—11. Remarks on ſome Antiquities on the Weſt and South Coaſts of Ceylon: written in the Year 1796.—12. On Mount Caucaſus.—13. On the Antiquity of the Surya Siddhanta, and the Formation of Aſtronomical Cycles therein contained.—Appendix. Rules of the Aſiatic Society. Members of the Aſiatic Society.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ASIATIC SOCIETY.

THE Aſiatic Society inſtituted in Bengal for inquiring into the Hiſtory and Antiquities, the Arts, Sciences, and Literature of Aſia, having reſolved to give with each ſubſequent volume of their Reſearches a liſt of ſuch oriental ſubjects as require further illuſtration, and to invite communications on the ſubject, have ſelected the following, and prefixed them to their laſt volume, lately publiſhed at Calcutta.

I. Religion, Policy, Juriſprudence, Manners, and Cuſtoms.

1. An accurate deſcription of the different feſtivals and faſts prevalent in India; together with an inveſtigation of their origin, and of the reaſon and ſignification of their peculiar ceremonies. As thoſe are very numerous, the following are ſpecified as objects of primary inquiry:

Among the Hindus—Doorga Pooja, or Dufferah; Kalee Pooja, Dewalee, Jonmon Aſhtomer, Churkh Pooja.

Account of the pilgrimage to the temple of Jagannatha, at Purſotom.

Among the Muſulmans—Eed ul Zoha, Eed ul Fetr, Eed Ghudeer.

2. An enumeration of the different caſts of Hindus, with the cuſtoms peculiar to each, as exiſting in the preſent time. —See an enumeration from the antient Sanſcrit records, Aſiatic Reſearches, Vol. V. p. 53.

3. A connected hiſtory of the ſeveral Muſulman tribes exiſting in India: among theſe, an account of the ſingular tribe known by the name of *Borah*, is particularly required.

4. What

4. What kinds of oaths are considered as peculiarly binding by the different tribes and sects in Hindustan?

5. What historical monuments remain of the government, and the system of police which obtained in Hindustan previously to the Musulman invasion?

II. Geography.

1. A catalogue of the names of towns, countries, provinces, rivers, and mountains, from the Schasters and Puránas, with their modern names annexed; and a correct list, according to the oriental orthography, of the towns, &c. mentioned by Major Rennell and other European geographers. The etymology, as far as practicable, would also be desirable.

2. What were the geographical and political divisions of the country before the Musulman invasion?

III. Biography.

1. Accurate translations of the accounts given of the life and actions of Bouddha, by the priests of his sect.

2. To inquire if there be any accounts remaining of Chauchasan, Gonagom, and Gaspa; whom the Burmas represent as having preceded Godama.

3. The history of Mahamoony, a disciple or follower of Godama; to whom also adoration is paid by many among the worshippers of Bouddha.

4. A history of those saints, philosophers, &c. either male or female, who have become famous, in modern times, among the nations and religious sects that inhabit India.

IV. Commerce, Natural History, Materia Medica.

1. To inquire into the state of the commerce of India previously to the first settlement of Europeans.

2. To ascertain the different trees which produce *gamboge*, or a gum-resin resembling it: to investigate the qualities of the drug as procured from each of those trees, among which we may reckon the following:—*Cambogia gutta*, Linn.; *Garcinia celebica*, Linn.; *Stalagmitis cambogioides*, Kœn.; *Hypericum pomiferum*, Rox. To procure accurate figures of

the *ſtalagmitis cambogioides*, or the Ceylon tree, and of the tree which yields this drug in Cambodia. Laſtly, to determine whether all thoſe trees may not be referred to one genus.

3. To aſcertain from what country the root commonly called *columbo* is procured, and to give a botanical deſcription and figure of the plant to which it belongs.

4. The botanical names of plants mentioned in the Hindu books of *Materia Medica*.

5. To ſupply the deficiencies which remain in the accounts of the production of borax in the neighbourhood of Tibet and Napal, as delivered by Mr. Blane and Father Joſeph de Ravato in the Philoſophical Tranſactions, Vol. LXXVII.

6. Whether the tobacco plant was known in Aſia before the diſcovery of America; and whether the edict, ſaid to have been publiſhed by Aurunzebe, againſt the uſe of that plant, be authentic?

V. Medicine and Surgery.

1. Hiſtory of that peculiar inflammation of the ſchneiderian membrane termed *nakra*, with the mode of treatment by the natives.

2. Hiſtory of inoculation for the ſmall-pox among the *Hindus*.

3. Antiquity of the venereal diſeaſe in India; and the knowledge which the antient Hindu phyſicians had of its cure.

4. Their treatment of the leproſy; with ſome account of the different ſpecies of that diſeaſe which are met with among the natives of India.

5. How long have the natives poſſeſſed the art of couching for a cataract, and from what ſource did they obtain it?

VI. Language, Literature.

1. How many dialects are there of the Hinduwee, *i. e.* of languages connected with the Sanſcrit; and in what parts of India were they or are they ſpoken?

2. What general term had the natives of India before the Muſulman invaſion, to deſignate what we imply by the term *Hindu*.

3. To obtain as full a catalogue as poſſible of books in the Sanſcrit

Sanſcrit and other Hinduwee languages, containing the following particulars, as far as they can be aſcertained, *viz.* the names of the authors, the ſubjects, the dates, the age of the moſt antient manuſcript of each now known to exiſt; and the places where the books are now to be found.

ELECTORAL ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

In the ſitting of February the 4th, Dr. G. H. Thilow communicated to the Academy an account of ſome experiments he had made in regard to Galvaniſm. Theſe experiments were a continuation of his former ones *, and tend ſtill further to confirm the idea that ſaltpetre poſſeſſes the peculiar property of weakening the nervous ſyſtem. This he aſcertained by ſeveral experiments. Having cut off a portion of the crural nerve, which had been laid bare, and weakened or deprived of its irritability by ſaltpetre, he laid bare another portion towards the knee, and, by ſtrewing nitre over it, deprived it in a few minutes of all ſuſceptibility of irritation. The ſame thing always took place as far as the toes whenever a new portion of the nerve was laid bare. The author thence concludes that the vital power is not totally extinguiſhed even when it cannot be excited by the ſtrongeſt ſtimulants applied to a nerve which has been ſeveral times ſubjected to the action of ſome ſtimulant. The heart of a frog laid bare, and ſtill in connection with the ſurrounding parts, and which contracted and expanded with great vivacity, being ſtrewed over with a little ſaltpetre, its movement was by theſe means immediately checked: though the author had taken the heart from the thorax, the fore-legs of the animal ſtill moved with ſuch ſtrength, that the whole upper part of the body was thereby agitated from one ſide to the other. Some drops of warm milk, let fall between ſome of the muſcles and nerves, ſpeedily put an end to this violent movement. Theſe ſtriking phenomena the author endeavoured to explain from the circumſtances of milk removing obſtinate ſpaſmodic affections, and reſtoring the equilibrium of the muſcular and nervous fibres. He had obſerved alſo,

* See *Philoſophical Magazine*, Vol. VI. p. 234.

ſeveral

ſeveral times, that perſons, whoſe fibres were too rigid and tenſe, were often troubled with crampiſh affections, and frequently ſuffered from violent flatulencies. He always found that their complaints were increaſed by bitters or carminatives in general, but that they were leſſened by the uſe of lukewarm milk.

The ſame author read ſome obſervations on the uſe of the German opium. As foreign opium is brought to Europe in an impure and imperfect ſtate, it is of importance to diſcover ſome ſubſtance that might be ſubſtituted in its ſtead. Profeſſor Trommſdorff found ſuch a ſubſtitute in poppy-heads, from which he has prepared a tincture. Dr. Thilow having made ſome experiments with this tincture in the Clinical Inſtitute of Erfurt, obſerved that it fully anſwered expectation in caſes of flux, ſpaſms in the ſtomach, gout, and hyſterics*.

FRENCH NATIONAL INSTITUTE.

C. Olivier lately read before this ſociety ſome obſervations on the jerboa.

His object was to rectify an error into which the antients and moderns have fallen, in regard to the manner in which the jerboa walks; and to make known the very ſingular organization of the genital parts of the jerboa, *mus jaculus*, LINN., *mus ſagitta*, PALL., to compare them with thoſe of the *alactaga*, and to deſcribe more at length the ſpecies mentioned by Linnæus, under the name of *mus longipes*, improperly confounded with the *mus meridianus* of Pallas.

The jerboa is repreſented in an erect poſture on the Cyrenean medals; Herodotus, Ariſtotle, Theophraſtus, Pliny, &c. among the antients, and Paul Lucas, Buffon, Allamand, Pallas, Vic. d'Azyr, &c. among the moderns, have all conſidered this ſmall quadruped as a biped, that is to

* It may be gratifying to our readers to learn that a premium for the production of opium in England, offered by the Society for the Encouragement of Arts, Manufactures, and Commerce, was awarded in their laſt Seſſion to a gentleman who produced a conſiderable quantity from poppies, which was found, by proper trials, to contain all the requiſite properties.

ſay, an animal which walks on its hind legs alone. Olivier has rectified this error by his own obſervation, which, on this point, perfectly agrees with the ſtructure of the body of the animal, as it does not allow it to remain long upright on its tarſi.

The penis of this quadruped, as obſerved by Sonnini *, is furniſhed with two long oſſeous hooks near each other, and placed towards the middle of the upper part of the glans. The latter is guarded beſides with *papillæ*, almoſt of an oſſeous nature, bent back and ſhaped like a ſpoon. In the alactaga, the *mus jaculans* of Pallas, the glans is merely covered with *papillæ*, in the form of ſpikes, almoſt ſtraight, rounded and directed backwards. The teſticles are concealed in the abdomen; and the orifice of the vulva in the female appears to be confounded with that of the anus.

The deſcription which Olivier gives of a ſmall ſpecies found by him in Egypt, and which, in ſize, is nearly equal to that of a rat, is perfectly applicable to the *mus longipes* of Linnæus; except that the latter, according to Linnæus, has only four toes on the fore feet, while that of Olivier has five; but it may be poſſible, ſays Olivier, that Linnæus did not pay attention to the thumb, which is indeed very ſhort.

As there is great confuſion in the ſynonymy of authors, Olivier endeavours, at the end of his memoir, to rectify their miſtakes. At the ſame time he gives the ſpecific characters of the ſpecies, which, in his opinion, belong to that genus.

1. Dipus *cerfer* pedibus poſticis tetradactylis.

2. Dipus *jerboa* pedibus poſticis tridactylis.

3. Dipus *alactaga* pedibus poſticis pentadactylis, lateralibus multo brevioribus.

4. Dipus *gerbillus* ſupra flavus ſubtus albus; pedibus poſticis pentadactylis, digitis ſubæqualibus.

Decandolle communicated to the Firſt Claſs of the Inſtitute a monography of the bilocular leguminous plants.

The bilocular leguminous plants are thoſe, the fruit of which are divided into two cells by a longitudinal partition, com-

* *Voyage en Egypte*, Vol. I. p. 133.

plete

plete or incomplete. Tournefort has given a ſection of them in his family of the *papilionacei*. Linnæus has eſtabliſhed them into three genera: the *biſſemla*, characteriſed by its indented ſeed; the *phaca*, the ſeed of which ought to be ſemi-bilocular; and the *aſtragalus*, where the ſeed is bilocular; but theſe two laſt genera are not ſufficiently diſtinct; for that reaſon Lamark united them. Decandolle has retained that arrangement, but he has changed the characters and rendered them more diſtinct.

The genus of the *phaca* has for its character a carina, furniſhed with a long ſtraight point; a ſeed with two longitudinal cells, ſometimes complete, but for the moſt part incomplete, formed by the folding back of the upper future. This genus comprehends twenty ſpecies, viz. *phaca ſebirica*; LINN.; *ph. myriophylla*, *muricata*, *ſylvatica*, *oxyphylla*, *proſtrata*, PALL; *aſtragalu verticellaris*, *alpinus montanus*, *campeſtris*, *piloſus*, LINN.; *aſtr. fœtidus*, VILL.; *aſtr. deflexus*, PALL.; *aſtr. annularis*, FORSK.; and ſeven undeſcribed ſpecies. The *phaca alpina*, *auſtralis bætica* and *rigida*, are referred to the *colutea*. The genus *aſtragalus* is diſtinguiſhed from the preceding by its *obtuſe* carina, and its ſeed with two longitudinal cells, ſometimes complete, or for the moſt part incomplete, formed by the *repli* of the *lower* future. It comprehends 125 ſpecies, 36 of which are nondeſcript. This vaſt genus had been divided into three ſections, according as the ſtalk was herbaceous, ligneous, or null; but theſe diviſions are not correct. Decandolle has divided the genus into two ſections, according as the ſtipulæ are diſtinct from the petiole, or adherent to it. The firſt ſection comprehends the ſpecies with yellow or purpurine flowers. The ſecond is divided into three ſubdiviſions; the firſt has the ſtipulæ adherent to the petiole and the ſtalk, and the petiole herbaceous and not ſpinous; the ſecond has the petiole not ſpinous and *caduque*; the third has the petiole ſpinous *perſiſtant*, and the ſolioles *caducous*. Theſe are the tragacantha of which the old botaniſts made a genus; but the fructification preſents no character, and exhibits the ſame variations as thoſe of the other aſtraguli.

The *phaca*, *aſtraguli*, *baguenaudiers*, and ſeveral other

genera in different families, have the verſicular pericarpium filled with air. Decandolle has obſerved, that if this air be analyſed at the moment when it is collected from the pericarpium, it is found to be of the ſame purity as atmoſpheric air; but if this pericarpium be immerſed in water, the air loſes its purity, and in the courſe of about a day no oxygen gas is found in it. The ſame thing takes place in the ſun as well as in obſcurity. The total quantity of the air does not appear to the eye to be diminiſhed. Is the oxygen gas converted into carbonic acid gas? or, rather, does it not ſerve for the nutrition of the ſeed. What tends to ſupport this idea is, that Humboldt found that the tunics of the ſeeds contain azot almoſt in a ſtate of purity. This fact coincides with an obſervation which Decandolle made before on the veſicles of the *fucus veſiculoſus*; he obſerved that theſe veſicles contained atmoſpheric air when they had remained ſome hours out of water, and azotic gas when they had been ſome time immerſed in it. Theſe facts deſerve the attention of phyſiologiſts.

It is well known that gum tragacanth is furniſhed by certain ſpecies of the *aſtragalus*, called on that account *tragacanthæ*. It appears that ſeveral ſpecies of this diviſion poſſeſs the ſame property; according to Tournefort, the *aſtragalus creticus* furniſhes this gum in the iſland of Crete; and the *aſtragalus gummifer* furniſhes ſome of it alſo in mount Lebanon, according to Lubillardier. It appears that it was the *aſtragalus echinoides*, from which Proſper Alpinus, as he ſays, ſaw this gum diſtilling. In a word, Olivier aſſures us that the gum uſed in commerce comes neither from Crete nor from Lebanon, but that the place of its deport is Aleppo; and that it is brought thither from Perſia, where indeed the ſhrub that produces it is found. It is a *tragacantha* not yet deſcribed.

The anti-ſyphilitic properties of the *aſtragalus exſcampus* have of late been much extolled, and ſtill require confirmation. The rarity of this plant prevents it from being ſubjected to experiments. Decandolle recommends making them on the *aſtragalus incanus*, and *monoſpeſſulanus*, which grow in France, and which ſeems to be analogous to the former.

The

The exiſtence of ornitholites in ſtrata of ſubmarine formation, is ſtill conteſted by ſeveral naturaliſts. The celebrated Fortis has even publiſhed a memoir in which he proves that no well aſcertained inſtances of this circumſtance had ever been known. Mention had indeed been made in ſeveral works of thoſe found at Montmartu; but ſome doubts ſtill remained.

C. Cuvier, however, has lately preſented to the Inſtitute a foſſil which ſeems to have all the characters of an ornitholite. It is a leg compoſed of a portion of the femur, a tibia, and a tarſus, in one piece; three toes, one of which has three, the ſecond four, and the other five articulations, and the veſtige of a ſpur. Theſe members are not found but in the claſs of birds alone.

This leg was incruſted in that kind of gypſum which in large ſtrata occupy ſuch an immenſe ſpace around Paris. It was found at Ville-Juif, in the third maſs, that is to ſay, about ſixteen yards lower than the ſtrata which contain the bones of quadrupeds already deſcribed by the ſame author.

C. Vidron, muſic maſter at Paris, having announced a diſcovery he had made, by means of which perſons born deaf could be enabled to hear muſic. Haüy, Lacepede, and Cuvier, were appointed to examine this diſcovery, and to make a report on the ſubject to the Inſtitute. It appears from this report that the apparatus employed by C. Vidron conſiſts of a ſteel rod, one end of which he places on the lid of a harpſichord, and the other between the teeth of the deaf perſon. This rod is furniſhed alſo with a branch terminated by a knob, which reſts in the pit of the ſtomach, and ſometimes alſo with a ſecond, applied to the cranium. The commiſſioners mentioned that ſeveral authors have announced that certain deaf perſons had been made to hear muſical ſounds by eſtabliſhing a communication between their teeth and the inſtrument by means of a rod, goblet, or other body. Of theſe authors they mentioned Fabricius ab Aquapendente, Schellhammer, Boerhaave, Winkler, and Joriſſen.

They endeavoured, in particular, to determine how far this invention might be of utility either in regard to the different kinds of deafneſs, or in regard to the different kinds of ſound, which

which one may wiſh to communicate. They gave themſelves an artificial kind of deafneſs by ſtopping their ears, and removing to a great diſtance. In both theſe caſes, ſounds were perfectly heard by the ſteel rod, and the ſounds appeared to them to come from the inſide of the rod, and not from their real place. But the reſults were different with reſpect to ſome perſons actually deaf, on whom the experiments were tried. Some of them heard very diſtinctly; but the greater number declared that they experienced only a tremulous movement, more or leſs general.

The commiſſioners conclude that this method may be employed with ſucceſs in caſes of deafneſs which ariſe only from ſome obſtructions in the external part of the auditory paſſage, but that it is uſeleſs in thoſe occaſioned by a paralyſis of the nerve, or any eſſential derangement in the interior organs, which, unfortunately, is moſt commonly the caſe, eſpecially in thoſe born deaf. They are, however, of opinion, that it ought to be tried on young perſons who are deaf; for, even if there were found only one in a hundred capable of deriving benefit from this reſource, it would at any rate be one ſource more of enjoyment to ſuch an individual.

In regard to articulate ſounds or words, the commiſſioners found that it is hardly poſſible to hope that they can be conveyed with accuracy by this apparatus, at leaſt in its preſent ſtate.

In the ſitting of Auguſt 23, General Dugua, who had returned from Egypt, having brought with him two copies of a remarkable inſcription found on a black granite of an exceedingly fine grain, preſented them to the Inſtitute.

This monument was diſcovered in digging up the earth at Fort Elleve, near the Bogar of Roſetta, two leagues from that town.

It was not brought to Cairo till after Buonaparte's departure, and was depoſited in the Inſtitute of that city. The diſcovery was made by C. Bouchard, lieutenant of engineers, who was appointed to ſuperintend the repairs of the fort.

Two members of the commiſſion of the arts were employed at Cairo in taking copies of this monument, unique of its kind, in order that it might be preſerved whatever might

happen. The firſt was C. Marcel, director of the national printing-office in Egypt, who poſſeſſes extenſive knowledge and an indefatigable zeal for acquiring more. By a ſimple typographic proceſs he obtained copies with the characters inverted, but which could be eaſily read by means of a mirror. The ſecond was C. Conté, chief of the brigade of aëroſtats, who, by his knowledge of mechanics and the arts, had always been one of the moſt uſeful men in the colony.

He employed the proceſſes of engraving to obtain copies, which at firſt gave the characters inverted; but theſe copies being put under the preſs, gave others ſimilar to the inſcriptions.

The inſcription is in three different characters; one portion preſents a ſeries of hieroglyphics, in ſeveral lines exceedingly regular; another which has not yet been ſufficiently examined, exhibits a great number of lines in characters which ſtill leave ſome uncertainty, and which require a more profound reſearch. The laſt portion is compoſed of fifty three lines, written in Greek. Laport-Dutheil one of the members of the claſs of literature and the fine arts, immediately began to tranſcribe this part; and communicated to the claſs a part of its contents. He afterwards made known the ſubſtance of nearly the whole of it, having tranſcribed the firſt thirty-eight lines. According to his explanation, it appears that the inſcription is a monument of gratitude from the prieſts of one or more temples, either of Alexandria or ſome place in the neighbourhood, to Ptolemy Epiphanes.

A literary man of eminence has already publiſhed ſome obſervations on the firſt explanation given of the Greek text. If the inſcription be in honour of Ptolemy Dionyſius, ſays he, it follows that the date of it muſt be 63 years before the Chriſtian æra; on the other hand, if the inſcriptions expreſs the ſame thing, it thence follows that the hieroglyphic language was in uſe even ſo late as 200 years before Chriſt *, though

* The Apocalypſe furniſhes a direct proof that it was underſtood, even ſubſequent to the Chriſtian æra. If commentators had attended to this fact, and followed the hieroglyphical acceptation of the figures employed

though it is commonly believed among the learned, that in the time of Herodotus, that is nearly 500 years before Chriſt, the knowledge of this language was already loſt.

Since theſe inſcriptions were preſented to the Inſtitute, Bonaparte has ordered engravings to be made of them, in order to gratify the curioſity of the learned in all countries.

FOSSILS.

Faujas St. Fond has lately brought to Paris ſome curious foſſils, having on them the impreſſion of plants; which he found under lava at the depth of twelve hundred feet. Juſſieu, Des fontains, and Lamark, have obſerved that ſeveral of theſe impreſſions belong to plants common in Europe at preſent; ſuch as leaves of the cheſnut tree, of the birch, mountain-aſh, the maple, an entire cone of the *pinus ſylveſtris*, and a portion of a cone of the *pinus ſativa*. Fabricius and Latreille diſcovered in them alſo the impreſſion of an inſect, which is the common *hydrophylus* or water-beetle. Faujas St. Fond intends to publiſh an account of this diſcovery, illuſtrated with engravings.

ployed in that book, inſtead of giving their own opinions, founded on a ſuppoſed reſemblance of the figures to the objects to which they arbitrarily applied them, they would have come nearer to the author's meaning than they have done.—EDIT.

ERRATA IN VOL. VII.

Page 356, l. 21, for *this* r. *their*: p. 360, l. 5. for *her* r. *the*: p. 362, l. 11; for *mere* r. *more*: p. 372, l. 4, for *ſulphat*, r. *ſulphuret*.

THE PHILOSOPHICAL MAGAZINE.

NOVEMBER 1800.

A short View of the Observations which have been made at different Times on the Luminous Appearance of the Sea. Read in the Physical Society of Gottingen, by J. G. L. BLUMHOFF *.

THERE are many things in nature of which we know nothing more than that they exist: respecting the manner in which they exist, and for what purpose, we are entirely ignorant. They are often the objects of our wonder and admiration; but, owing to the imperfection of our faculties, we can acquire no satisfactory knowledge of their nature. The only course, therefore, which, as rational beings, we can pursue in such cases, is to endeavour, by the help of conjectures, to open a way to truth; and, when this cannot be accomplished, to endeavour to approach as near to it as possible. This, indeed, may often be effected with some degree of success, by admitting certain hypotheses, or fundamental principles deduced from analogy, especially when our researches are conducted with accuracy and perseverance. It may be proved by innumerable instances, taken from the history of the sciences, that hypothetical and conjectural explanations have often conducted mankind to the discovery of immutable laws, and truths of the greatest importance.

To explain the phænomena of nature, has always been

* From *Magazin für den Neuesten Zustand der Naturkunde*, &c. Vol. I. part 4.

the employment of philoſophers; and yet there are many of them reſpecting which our knowledge is very uncertain. But how is it poſſible for ſuch ſhort-ſighted beings as men are, to have at command, and employ in a proper manner, all thoſe external circumſtances which are neceſſary for the thorough examination of ſuch phænomena? A thouſand things occur daily in nature which it is impoſſible for our coarſe organs to perceive, and much more to obſerve in a perfect manner: in this paper, however, I ſhall not deſcend to microſcopic objects, but offer a few remarks reſpecting a phænomenon which frequently occurs, and which has attracted the notice of many celebrated philoſophers—I mean the luminous appearance of the ſea. This phænomenon, which exhibits ſo magnificent a ſpectacle to navigators, and which, when beheld for the firſt time, muſt excite aſtoniſhment, deſerves certainly to be examined with more care and attention. I ſhall here, therefore, take a ſhort view of the information which has been collected on this curious ſubject.

That the water of the ſea ſometimes exhibits a luminous appearance in the night-time, eſpecially when ſtruck by the rudder of a ſhip, or ſtrongly agitated by its reſiſtance to the motion of the veſſel, is a circumſtance well known; and this phænomenon appears ſometimes, in calm weather, like multitudes of ſmall ſtars diſperſed over the ſurface of the ſea. It frequently happens, alſo, that the places only next the ſhip or the ſhip's wake, or the track through which fiſh have paſſed, appears luminous. The well-known Americus Veſpuſius, the Italian navigator, according to Kircher, was the firſt perſon who obſerved this circumſtance.

In Lichtenberg's Magazine for the lateſt Diſcoveries in Phyſics and Natural Hiſtory*, we find the following deſcription of this phænomenon as obſerved in the Baltic:—"The phoſphoric phænomena in the Baltic generally appear in the darkeſt nights, with an undulating motion, in the track formed by veſſels in their progreſs through the waves. The water ſeems to emit a lively light, which is ſometimes of a pale red colour, and has the reſemblance of ſparks. Sometimes it exhibits the appearance of a regular ſtream of fire in

* Vol. II. part iv. p. 48.

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the ſhip's wake. This phænomenon is ſeen ſometimes alſo when the wind blows, but not with that majeſtic appearance as during a calm. It is worthy of remark, that, when veſſels are lying at anchor, this phænomenon is obſerved here and there, darting itſelf from one place to another with great vivacity, behind the veſſels, even when they make little or no motion." This deſcription agrees in the moſt eſſential parts with that of Gentil, who obſerved the ſame phænomenon in the Indian ſeas, particularly in the channel of Mozambique. On one occaſion the ſea appeared to this navigator and his whole crew to be entirely covered with fire, and each wave to conſiſt of a maſs of phoſphoric matter. The ſhip ſeemed as if moving up and down in a fiery lake. Some of the ropes reflected the light ſo ſtrongly, that the people on board could have read by it. The fire of Saint Elmo appeared alſo at the ſame time on the ſummit of the mainmaſt *.

Boyle † ſought for the cauſe of this phænomenon in certain general laws of the earth, or of our planetary ſyſtem; but, had this celebrated philoſopher conſidered the ſubject with a little more attention, he would have found that it may be explained on much better principles.

Father Bourzes ‡, during his voyage to India in the year 1704, had ſeveral times an opportunity of obſerving the luminous appearance of the ſea, and has given us a very valuable account of the phænomenon. The light was often ſo ſtrong that he could read by it the title of a book, though he ſtood at the height of nine or ten feet above the ſurface of the water. Sometimes he could diſtinguiſh the luminous

* Nous eumes dans ces parages de ſi mauvais temps, et entr' autres une nuit ſi épouvantable, que la mer ſembloit être tout en feu: chaque lame étoit pour ainſi dire un phoſphore: le vaiſſeau paroiſſoit être dans un étang de feu. Nous étions à la cape ſous la miſaine: cette voile refléchiſſoit la lumière de la mer à un point, qu'on eût dit qu'elle étoit éclairée par la lumière d'un très grand nombre de fanaux; et on eût pu lire auprès de la relinque de cette voile, à la faveur de cette lumière refléchie de la mer. Le feu de St. Elme parut un inſtant au haut du grand mât.—*Voyage dans les Mers d'Inde*, Vol. I. p. 687.

† Philoſophical Tranſactions, Vol. LIX. p. 450.

‡ Lettres édifiantes, Vol. IX. Philoſophical Tranſactions abridged, Vol. V. p. 213.

parts from thoſe that were not ſo in the ſhip's wake. The former exhibited in part luminous points and in part ſmall luminous globules, ſome of which were from one to two lines in diameter, and others as large as a man's head. Sometimes they formed ſtreaks from three to four lines in length, and one or two lines in breadth: ſometimes they reſembled luminous vortices, which, according to the author's expreſſion, appeared and diſappeared ſuddenly at certain periods, like flaſhes of lightning.

Father Bourzes obſerved alſo, that the paſſage of fiſh through the water was marked by a ſtream of light, ſo that it was often poſſible to diſtinguiſh their ſhape and ſize. He remarked alſo a number of luminous particles in the water drawn up from the ſea, when he ſtirred it round with his hand in a dark place. Theſe luminous particles he obſerved alſo on a piece of linen which he had dipped in ſea-water, when it was wrung in the dark, and even when it was half dry.

This attentive obſerver conſiders the principal cauſe of theſe phænomena to be a kind of greaſy viſcous matter in the water of the ſea, which ariſes perhaps from putrefaction, becauſe he thought that the water, the fatter it ſeemed, and the more covered with ſcum, always emitted the more light. In confirmation of this opinion he ſtates, that having caught a bonetta, its mouth appeared ſo luminous in the inſide that he could ſee to read a book by it. On examination, the mouth of the fiſh was found to be full of a viſcous matter, which being rubbed over a piece of wood, the latter became alſo luminous in the dark, but the light it emitted ceaſed as ſoon as the matter became dry.

The experiments of Canton, mentioned in the Philoſophical Tranſactions *, ſeem to coincide perfectly with the above deſcription of father Bourzes. I ſhall therefore give a ſhort account of them, as they are the moſt accurate we have on the ſubject. On the evening of the 14th of June 1768, Mr. Canton put ſome freſh whitings into ſea-water, and found that they emitted light for twenty-four hours. In the cellar where he depoſited the veſſel which contained

* Vol. LIX. p. 446.

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them, Fahrenheit's thermometer ſtood at 54°. The water itſelf, when at reſt, was quite dark; but, when a ſtick was drawn through it, the whole part through which the ſtick had paſſed became luminous, and no light appeared any where elſe. When he agitated the whole water, it became luminous throughout. When the fiſh had lain in the water forty-eight hours, it was then brighteſt: at the end of three days, though ever ſo much agitated, it emitted no more light.

The ſea water, however, exhibited a much more luminous appearance when a herring was placed in it. The third night it was ſo ſtrong, that, when ſtirred, a perſon could tell by it the hour on a watch; and at thoſe moments the fiſh ſeemed to be an opake body. After this period the light decreaſed more and more, but it ſtill continued, in ſome degree, till the ſeventh night.

Another veſſel containing freſh water, into which a freſh herring was put in the like manner, continued entirely dark, without emitting the leaſt trace of light. The thermometer during the time ſtood always above 60°.

River water, in which ſalt was diſſolved till it appeared by a hydrometer that it had acquired the gravity of ſea water, exhibited the ſame phænomena as water brought from the ſea: in water to which more ſalt was added, the fiſh emitted no light.

It appears from theſe experiments and thoſe of Sir John Pringle* that the ſalt in ſea water promotes putrefaction; and that therefore the luminous appearance of ſea water ariſes from a diſpoſition to putrefaction, or a commencement of it.

Experiments on the ſhining of fiſh in ſalt water have been made alſo by other philoſophers beſides Canton: among theſe we may mention thoſe of Boyle and Dr. Beal in the Philoſophical Tranſactions †, and thoſe of Martin in the Tranſactions of the Swediſh Academy ‡, all of which ſeem to confirm, in a greater or leſs degree, that the luminous appear-

* Experiments on Septic and Antiſeptic Subſtances.

† Philoſophical Tranſactions, No. 31, p. 581; and No. 13, p. 226.

‡ Swediſh Tranſactions, Vol. XIII. p. 225.

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ance of ſea water is owing to a commencement of putrefaction *.

Another circumſtance which has been proved by theſe obſervations is, that heat deprives putrid bodies of their luminous property. This circumſtance did not eſcape Canton; for he remarked that, though a ſtrong ſummer heat promoted putrefaction, a heat twenty degrees higher than that of the blood ſeemed to prevent it. This he diſcovered by the following experiment:—He put a ſmall piece of a fiſh which had become luminous, into a globe of thin glaſs, immerſed it in water heated to 118° of Fahrenheit, and obſerved that it was deprived of its luminous property in leſs than a minute: on being taken from the water, however, it recovered its luminous property in ten ſeconds, but the light it emitted was not ſo ſtrong as before.

Spallanzani does not agree in opinion with Canton, that the luminous appearance of fiſh ariſes from putrefaction †. He repeated Canton's experiments with a variety of fiſh, but he ſucceeded only with a very few of them. Thoſe which did not emit light he found in general to be the fatteſt, though, according to Canton, theſe ought, above all others, to communicate a luminous appearance to ſea water. Another circumſtance, which ſeems contrary to Canton's hypotheſis, is, that if the nocturnal luminous appearance of the ſea were occaſioned by the oleaginous remains of putrid fiſh, as theſe remains always float at the ſurface the light ought to be confined to the ſurface of the water; which experience ſhows not to be the caſe.

Vianelli, a phyſician of Chioggia in the neighbourhood of Venice ‡, Grizzelini, a Venetian phyſician, the abbé Nollet §, and Dr. Prieſtley, aſcribe the luminous appearance

* That ſeveral kinds of fiſh, ſuch, for example, as the *ſepia*, ſhine in the dark, is well known.

† Memorie di Matematica e Fiſica della Soc. Italiana, Vol. II. p. 603.

‡ Nuove Scoperte intorno le Luci notturne dell' Aqua Marina.—*Venez.*

§ Nouvelles Obſervat. ſur la Scolopendre Marine, and the abbé Nollet, in the Memoires de l'Acad. des Sciences 1750, p. 88.

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of the ſea to phoſphoric inſects *. The abbé Nollet, in particular, while at Venice in the year 1749, paid great attention to this phænomenon, and was confirmed in his opinion by the following obſervation:—Having remarked that the ſea water in the harbour of Portoſino emitted a ſtrong light, he lay down on the edge of the water; and, ſtretching his head over it, he ſaw theſe inſects ſpring up, as inſects in general do, from the bottom of the water, which was covered with ſea-weeds. When he attempted to catch them in his handkerchief, he found on it only luminous ſpots, which he could ſpread over it with his finger.

Le Roi, during a voyage up the Mediterranean, obſerved that the head of the ſhip, as it paſſed through the waves, threw up in the daytime a multitude of ſmall particles, which at night had a fiery appearance †. He is not, however, inclined to aſcribe this phænomenon to inſects, becauſe, having collected ſome of theſe luminous particles in his handkerchief, he found them to be round bodies like pin-heads, without any appearance of animal organs, though he examined them with a microſcope. But he does not deny that there are luminous inſects in the ſea, though he is of opinion that its luminous appearance ariſes from ſome other cauſe, which he does not further explain.

Fougeroux de Bondaroy ‡ and father Torrubia § aſcribe the cauſe of the luminous appearance of the ſea to a multitude of ſmall nereids, the hinder parts of which the former found to be endowed with the property of emitting light. This is the caſe in particular with the *nereis noctiluca* Linn. ‖

* Some of the ſcolopendræ, as the *ſcolop. electrica* and the *ſcol. phoſphorea* L. really ſhine in the night-time. According to Hablitzl (ſee *Goth. Mag.* Vol. II. Part 4. No. 159), a cable drawn up from the ſea emitted a very lively light, which aroſe from its being covered with the ſea flea (*cancer pulex*); a proof that thoſe animals ſhine in the dark.

† Obſervat. ſur une Lumière produite dans la Mer, in the Memoires preſentées, Vol. III. p. 144.

‡ Sur la Lumière que donne l'Eau de la Mer principalement dans les Lagunes de Veniſe, in the Mem. de l'Acad. des Sciences. *Paris* 1767, p. 120.

§ Vorbereitung zur Naturgeſchichte von Spanien. *Halle* 1773. 4to.

‖ See Linn. Syſt. Nat. Vol. I. edition 13. p. 1085; alſo his Amœnitat. Acad. Vol. III. p. 203. tab. 3.

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The celebrated Forſkäl, who accompanied Niebuhr as naturaliſt in his travels through Arabia, having caught a great number of marine animals, among which there were different kinds of *meduſæ*, put ſome of them into a bucket full of water. After keeping them ſome time, he threw the contents of the bucket from a window, in the dark, and obſerved that all thoſe places where the water fell ſeemed to be covered with ſparks of fire. He made ſeveral experiments on this ſubject, all of which convinced him that the luminous appearance of the ſea ariſes from inſects. Bartholinus alſo, and Donati, aſcribe the luminous appearance of the ſea to marine worms (*moluſcæ*).

Silberſchlag aſcribes the luminous appearance of ſea water to phoſphorus, the component parts of which may exiſt in the ſea as well as in the atmoſphere. It is not improbable that ſeveral kinds of worms, which, according to various obſervations, have been conſidered as luminous, may be indebted for that property to marine phoſphorus adhering to them. It has been found, by repeated obſervations, that the water of the ſea, even at the depth of forty feet, is phoſphoreſcent; and the abbé Spallanzani is therefore inclined to believe that this is the caſe alſo at every depth. Reſpecting the luminous appearance of the ſea water, however, he gives no deciſive opinion, and conſiders every thing hitherto ſaid on the ſubject as mere conjectures.

Mr. Erich Schytte remarks, that when ſea water is converted into ice it retains its luminous property; and that, when ſea water is diſtilled, the luminous matter does not paſs over, but remains behind in the ſtill.

Profeſſor J. Mayer, in a paper publiſhed in the Tranſactions of the Royal Bohemian Society on the luminous appearance of the Adriatic, ſays, that the water at the ſurface ſeems to imbibe the rays of light in the ſame manner as a light magnet. This luminous appearance is obſerved more at the ſurface than at any depth in the water, and diſappears when, by violent agitation of the waves, the water at the ſurface becomes mixed with that below. This author, however, is of opinion, that there are in the water foreign particles which emit a ſtronger light, and which may be ſeparated

from it, but there are no proofs that electricity has any ſhare in the phænomenon.

M. Bajon, on the other hand, conſiders the luminous appearance of the ſea as electric, and, in ſupport of this opinion, remarks that it is never obſerved but when friction is produced by ſome body paſſing over or through the water. For this reaſon the light is always ſeen very ſtrong around ſhips, and where great numbers of fiſh are collected together. The action of the atmoſphere may alſo excite this light at the ſurface of the ſea. It is promoted by a north wind, and is interrupted by a ſouth wind, as well as by damp weather, &c. This opinion, in the moſt eſſential parts, agrees perfectly with that of M. De la Perrier, and of J. R. Forſter.

M. Le Gentil alſo concludes, from obſervations made in the Indian ſeas, that this phænomenon is produced by the electricity of the ſea water; for he obſerved it only under certain caſes; as for example, when the heavens were filled with clouds, and the ſea high, and much agitated. He obſerved it, however, during calms; but this was never the caſe except when a ſtorm was to be apprehended. As long as the ſtorm continued, the ſea had a luminous appearance; but as ſoon as it was over, the light ceaſed. When the weather was in the uſual ſtate, that is to ſay, when the winds were regular, even though they were ſtrong and the ſea rough, he obſerved nothing of the phænomenon. He ſailed upwards of ſix hundred leagues with a ſtrong weſt wind and very high ſea, but did not obſerve the leaſt ſpark of light. They appeared moſt abundant in winter, which is the period when the winds change, and when ſtorms and hurricanes take place. M. Le Gentil aſſerts that at this period of the year he never wiſhed to obſerve the ſea luminous, becauſe he conſidered it as a ſign of bad weather, and could always, with certainty, foretel by it when a change was to take place. This phænomenon is more common in the Indian ſeas than in thoſe parts on this ſide of the Cape, between America, Europe, and Africa. M. Le Gentil aſcribes the electricity of the ſea to the daſhing of the waves againſt each other, which muſt naturally produce friction.

I hope I ſhall be forgiven if I here add the following va-

luable obſervations on this ſubject by profeſſor Forſter, of Halle. This celebrated navigator and naturaliſt obſerved this phænomenon on the night of the 29th of October 1772, during a freſh gale, a few miles from the Cape of Good Hope. He ſaw alſo a great quantity of ſmall luminous bodies floating on the ſea, ſome of them near and others at a conſiderable diſtance from the ſhip. He cauſed ſome of the ſea water to be hauled up in a bucket, in which he found an immenſe number of ſmall globules which moved with great rapidity. When the water was ſuffered to ſtand ſome time at reſt, theſe luminous globules became leſs numerous; but, when agitated, they reſumed their luminous property. Mr. Forſter found, when he examined ſome of theſe globules, that they had life and organiſation, but they died before he could waſh them from his fingers.

Profeſſor Forſter diſtinguiſhes three kinds of light in the ſea, which ſeems to agree pretty well with the beforementioned obſervations. The firſt kind, he ſays, muſt be aſcribed to electricity, becauſe the quick motion of ſhips through the water, eſpecially when the wind is ſtrong, occaſions a violent friction; and becauſe the agitation of the waves produced by the wind heats the waves more than the atmoſphere above them. The reſin, pitch, and tar, with which the outſide of ſhips is covered, and the conducting property of water, render the probability of this phænomenon being connected with electricity ſtill ſtronger.

The ſecond kind of light appears, in a proper ſenſe, to be phoſphoric. A great many animal bodies, by putrefying in the ſea, become decompoſed, and conſequently their component parts, and particularly the phoſphoric acid, are diſengaged. An addition of inflammable matter forms with this acid that mixture which is commonly known by the name of *phoſphorus* *. Thus, fiſh which are dried in the air become ſometimes phoſphoric; and this is the caſe with the ocean itſelf, when, after a long calm, it has been filled with

* Profeſſor Forſter's reaſoning, here quoted, is agreeable to the old theory. We have no right to alter his words; and it will be ſeen by our chemical readers that the facts might as eaſily be accommodated to the modern theory.

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putridity and corruption; as heat and tranquillity contribute to promote the ſpeedier ſolution of animal ſubſtances. Fiſh, as well as gelatinous animals, contain oily and inflammable particles, with which the liberated phoſphoric acid may readily unite and form a phoſphoras on the ſurface of the ſea, and give riſe to this wonderful phænomenon.

The third kind of light ariſes, no doubt, from living animals which float in the ſea, and muſt be produced by their peculiar organiſation, or rather their component parts, which deſerve to be better examined by chemical experiments.

Theſe conjectures of Dr. Forſter contain, in my opinion, the moſt ſatisfactory explanation of this phænomenon. All the effects of it hitherto obſerved may be referred to ſome of the above-mentioned cauſes; and the general concluſion that may be deduced from them is, that as theſe effects have been ſeen under different circumſtances, they muſt be explained by different cauſes.

II. *On the Submerſion of Swallows in Autumn.*

IN our fourth Volume, p. 414, we laid before our readers an intereſting letter on this ſubject from Mr. Peter Cole to Dr. Mitchill of New York, to which we refer. Such relations are often queſtioned; but in natural hiſtory, as in other branches of ſcience, the evidence of poſitive facts is never to be controverted by mere inferences from other ſuppoſed or real phænomena. Mr. Cole is a gentleman whoſe veracity will not be queſtioned by thoſe who know him, and we have therefore no right to doubt the truth of former relations of a ſimilar nature merely becauſe we know not *now* the character of the relaters. The late Hon. Daines Barrington took the trouble to collect a number of teſtimonies of ſimilar ſubmerſions obſerved in England, which were publiſhed in the Philoſophical Tranſactions, and we ſubjoin another teſtimony of ſuch phænomena being obſerved in the New World.

"New York, 18th July 1800.

"On the afternoon of the 24th of Auguſt 1798, I was ſitting in my parlour, which looks towards the North

River*, about fifty feet from the bank, in company with our mutual friend Mr. Jacob Sebor. Our attention was attracted by numerous flights of birds, which appeared to come acroſs the town from the eaſtward, and deſcend immediately into the river. So ſingular an appearance excited our particular obſervation. We went out and ſtood cloſe to the bank, and then perceived that what we at firſt imagined to be black birds were actually ſwallows; and that, as ſoon as the various flocks had cleared the houſes, and got directly over the river, they plunged into the water and diſappeared. This was not confined to the vicinity of the place where we ſtood, but was the caſe as far as the eye could reach, up and down the river, and continued without ceſſation for nearly two hours, when the cloſing of the evening prevented our further obſervation.

"Aware of the importance of affording any additional information on this long diſputed queſtion in the natural hiſtory of the ſwallow, I procured a teleſcope, and watched attentively many of the flocks from their firſt appearance until their immerſion, continuing my eye fixed upon the ſpot long enough to be fully convinced that not one of the birds returned to the ſurface again. Indeed, one flock of about two hundred birds plunged into the water within thirty yards of us, and inſtantly diſappeared, without the leaſt appearance of oppoſition that might be expected to ariſe from their natural buoyancy; and at the ſame time the evening was ſo ſerene, and the river ſo unruffled, that no deception of our ſight could poſſibly have occurred.

"When the birds firſt came in view, after croſſing the town, their flight was eaſy and natural; but when they deſcended near to the water they appeared much agitated and diſtreſſed, flying in a confuſed manner againſt each other, as

* The houſe of Mr. Pollock is ſituated near the margin of the Hudſon, about two hundred yards from the battery. The river is about a mile and a half wide, and from ſeven to nine fathoms deep, and runs with a ſtrong and rapid tide. Mr. Pollock does not recollect the ſpecies of ſwallow which then diſappeared. The barn ſwallow (*hirundo ruſtica*), chimney ſwallow (*hirundo pelaſgia*), the ſand or bank martin (*hirundo riparia*), and the purple martin (*hirundo purpurea*), all frequent and build their habitations in this city and its neighbourhood.

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if the love of life, common to all animals, impelled them to revolt againſt this law of nature impoſed upon their ſpecies. 'As ſome time has elapſed ſince the above-mentioned facts occurred, I thought it proper, before I gave you Mr. Sebor's name, as having been a witneſs to them, to conſult his recollection on the ſubject, and I have pleaſure in aſſuring you he diſtinctly remembers every circumſtance I have recited, and of which I made a memorandum at the time.' It may be worthy of remark, that, as far as my obſervation went, the ſwallows totally diſappeared on the 24th of Auguſt 1798; for, during the remainder of that year, I did not ſee one.

"H. POLLOCK."

III. *Account of a fatal Accident which happened to a Traveller on the Glacier of Buet; with ſome Cautions to thoſe who through Curioſity may viſit the Mountains of Swiſſerland, and particularly the Glaciers.* *By* M. A. PICTET, *Profeſſor of Philoſophy.*

[Concluded from Page 61.]

C. D'EYMAR, in conſequence of a report from two officers of health, reſpecting the inconveniences which might ariſe if Eſchen's funeral ſhould be deferred, gave the neceſſary orders in our preſence for interring the body in a proper and decent manner. For his purpoſe we made choice of a ſpot which would neceſſarily be ſeen by every traveller going to Chamouni, and even by thoſe who might aſcend the glacier of Buet by the route of Servoz and Villy, in conſequence of the intention expreſſed by the prefect that a monument erected over the grave of this unfortunate young man, with a ſuitable inſcription, while it preſerved a remembrance of the event, might forewarn travellers of the dangers to which they would be expoſed in traverſing the glaciers without taking proper precautions, and without paying to the information of their guides that attention to which it ought to be entitled. While preparations were making for the interment, we interrogated C. Deville and his companions with much intereſt,

intereſt, reſpecting their expedition to the glaciers, and the following account is drawn up from minutes written down at the time:

" In conſequence of the order of C. d'Eymar, præfect of Leman, C. Joſeph Maria Deville, after cauſing a harpoon to be forged, and providing ropes, ſet out, accompanied by his two ſons John Claude and Bernard, and Joſeph Ettle, innkeeper at Servoz, in order to proceed to the glacier of Buet in ſearch of the ſtranger ſwallowed up by a fiſſure in the ſnow. They left Servoz at ſeven in the evening, and travelled the whole night.

" Having arrived on the glacier at day-break, they immediately repaired to a hut built of ſlate in the neighbourhood, called Caſtle Pictet, near which, as they were informed, the accident had taken place. They obſerved ſome ſigns of a fiſſure covered by the ſnow, but there was no aperture. They however continued their ſearch, and about two in the afternoon Deville found in the ſnow a hole almoſt ſquare, about two feet in extent each way, but of which they could not diſtinguiſh the bottom. In the neighbourhood there appeared ſome ſigns of the exiſtence of a fiſſure beneath the ſnow.

" By means of a ſtone tied to the end of a rope in the form of a ſounding-line, they were able to diſcover in this hole, at the depth of more than a hundred feet, the preſence of a body of a nature different from ſnow or ice. They then let down the harpoon, which ſeemed to have laid hold of ſomething, but which brought up only a ſingle hair. Bernard the ſon then propoſed to deſcend attached to a rope, which he accordingly did; and when he arrived at a depth where there were only eight inches diſtance between the ſides of the fiſſure, he was able to touch, with a ſtaff five feet in length, the head of the dead body below him. He then cauſed himſelf to be hoiſted up, becauſe he found his poſition exceedingly confined, without having power to move any of his limbs.

" The harpoon was again employed, but it brought up nothing except fragments of the clothes and a hat. Night came

came on, the weather was cold, and the travellers were wet. They were obliged, therefore, to return to the Chalets de Villy, where they arrived about ten at night.

" Having conſulted together, they reſolved to return to the glacier, after ſupplying themſelves with ſome pieces of wood and ſome more ropes. They ſet out from Villy at break of day, and on reaching the mouth of the fiſſure they conſtructed a kind of apparatus, by means of which Deville the father was let down; but he was ſtopped by the narrowneſs of the fiſſure, the ſame obſtacle which had ſtopped his ſon. He had carried down with him a ſhort-handled hatchet, by means of which he endeavoured to enlarge the paſſage towards the place where the body lay, in order that it might not be covered by the fragments of the ice. He deſcended gradually towards it, and endeavoured, but in vain, to move it, as it was ſo ſtrongly wedged in between the ſides of the pit. He then endeavoured to cut the ice around it, and at laſt diſengaged the upper part of the body in ſuch a manner as to be able to faſten a rope round it below the arms. The body was in an erect poſition, the arms raiſed up to the head, and the face turned towards the left ſhoulder. It was quite frozen.

" Deville then called out to the aſſiſtants to pull the rope, in order to try whether it was poſſible to raiſe the body, but the attempt was fruitleſs. He then continued to clear away the ice around, which he at length accompliſhed, after three hours labour with his hatchet. When this was completed, he firſt cauſed himſelf to be drawn up, and then the body; by this time it was five in the evening.

" Search was then made in the pockets of the deceaſed, in order to find out and collect all his property, and an inventory of them was ſubjoined to the preſent minutes. A ſledge was made with ſome pieces of wood, and in this manner the body was conveyed beyond the glacier. The brothers Deville then carried it on their ſhoulders, in turns, as far as the Chalets de Villy, where they arrived at ten in the evening. It was then placed on a mule, and, after reſting an hour, they again ſet out, and arrived at Servoz between five and ſix in the morning. Bernard, Deville's

youngeſt ſon, being worn out with fatigue, was obliged to remain behind at the Chalets de Villy."

Deville himſelf was entirely exhauſted; and, indeed, thoſe who reflect on the labour he had to endure for ſeveral hours in the confined place in which he was ſuſpended, and the length of the way he had walked, will be aſtoniſhed that he could hold out ſo long. He refuſed to take any repoſe until he had aſſiſted, along with us and a very numerous company, at the funeral obſequies of the unfortunate traveller. He was ſpecially charged by the præfect to cauſe a temporary pyramid to be raiſed in order to mark the ſpot.

The compreſſion which the unfortunate young man had experienced by an accelerated fall of more than a hundred feet, in a fiſſure the ſides of which approached each other in the form of a wedge, had been ſo violent that his watch was beaten flat. We found in his pockets 78 livres in money, and the third volume of Sauſſure's Journey through the Alps. Such of his effects as were of any value were given to Deville, according to the agreement made by M. Zimpffen, and the præfect engaged to tranſmit the reſt to his relations. We thought proper to add to them a lock of the hair of this intereſting youth.

In his pocket-book was found a letter which he had begun, written in German, and directed to his father. We cannot reſiſt the temptation of publiſhing ſome fragments of it; they diſplay a juſt ſpirit of obſervation, and great ſenſibility of mind: I hope his unfortunate parents will forgive us! It is the part which we take in their grief that renders us perhaps indiſcrete in publiſhing the letter of the ſon; but we wiſh to make our readers participate in their regret, and to announce, wherever this work may circulate, what kind of ſon theſe parents have loſt, what hopes have been ſnatched from them for ever *.

We

* The following is a literal tranſlation of this letter, dated Vevey, Auguſt 2, 1800:—"You ſee, my dear father, by the date of my letter, that I have undertaken a journey: you ſee alſo that this journey is one of the moſt intereſting that can be wiſhed.

"I ſet out on Tueſday from Rumlingen and on Wedneſday from Berne, and I ſhall not return to my preſent habitation before a fortnight or

We left Servoz a few minutes after the dismal ceremony, and arrived about midnight at Geneva, having our minds and hearts filled with the events of our journey.

One of the dangers to which travellers are exposed in these mountains must be already known to my readers; but this is not the only one, and a simple enumeration of the accidents which have taken place in the course of a few years back, will exhibit others which may sometimes occur.

One of our countrymen, young and active, having ventured without a guide, and without proper shoes, to traverse the steep and rocky declivities which form the base of the needle of Charmoz towards the glacier called *Des Bois*, was precipitated into one of these ravines, and perished.

A young inhabitant of Zurich having ascended to the

or three weeks, when I shall embrace my dear and beloved pupils, my Rudy and my Sophia. It is needless to tell you that I travel on foot, and you may also guess that I travel in company with a friend; for the heart and mind never enjoy the most beautiful aspects of nature but when conscious that a living being sympathises with us. Every thing beautiful and sublime, in order that it may make a deep impression on the soul of man, ought to be united with the enjoyment of love and friendship....

"Scarcely have you reached the canton of Fribourg when you lose sight of that opulence, civilisation, culture, and those natural beauties, which distinguish the canton of Berne; and the contrast is painful. I have found here a confirmation of the opinion I long before entertained of the old government of Berne. However deficient it might be in many respects, it was incontestably superior to all the other governments of Switzerland.

"In the hermitage (near Fribourg) there is now living an old hermit with a long white beard called *Le Frere des Bois*, a name given here to hermits. Though pretty far advanced in years, he is still very fresh. He served a long time as an officer in a Swiss regiment in the pay of Austria. We were very much surprised, when we ascended towards him, to see him coming slowly to meet us across the large hall, not in the dress of a hermit, but in a hussar uniform; his short open red cloak, bordered with fur, and his white breeches, formed a contrast with his long snowy beard. He received us with great kindness, spoke to us in French and German, complained a great deal of the depravity of the present age, and told us that it was not sufficient to pray, it was necessary to act, &c. He congratulated himself on the tranquillity of his life, his habitation being at a distance from all bustle, and his ears being assailed by no noise, except that occasioned by the fall of a neighbouring torrent, and the singing of birds."

ſummit of an inſulated rock which terminates the mountain of Baime to the north, and at a little diſtance from the neck or paſſage of that name which conducts from Chamouni to the Valais, becoming giddy, made a falſe ſtep, and was daſhed to pieces at the bottom of the precipice.

A Geneveſe family, who had gone to viſit the ice cave of Arveron, and who happened to be there at a time when the arch fell down in ſucceſſive fragments, in conſequence, it is ſaid, of a piſtol being fired, having been ſo imprudent as not to retire in proper time, while the accumulated ice obſtructed the torrent, one of the ſons was cruſhed to death by the falling blocks of ice, and hurried away by the waves; another was ſeverely wounded; and the father, after both his legs were broken, eſcaped from deſtruction only in a manner almoſt miraculous.

Theſe, however, are the only unlucky events which have taken place for a long time in a country viſited by many of the curious, who are ſometimes imprudent. It may readily be believed that none of theſe accidents would have happened, had thoſe who fell victims to them liſtened to the voice of common prudence, which would dictate nearly as follows:

That there is very little merit or glory in expoſing one's life, through oſtentation, in order to diſplay a courage in which the moſt common rope-dancer will always be ſuperior to the traveller who attempts to give proofs of his good head or agility in attempts more or leſs dangerous.

That no one ought to think of traverſing theſe mountains except when conducted by a robuſt, prudent, and experienced guide. Nothing can be more treacherous and deceitful than thoſe things which appear eaſy when ſeen at a diſtance; thoſe apparent paſſages, in which travellers are gradually involved, without reflecting that, if they are at laſt ſtopped by fear, or by the impoſſibility of proceeding further, this fear will double the difficulty of retreating, becauſe it deſtroys that coolneſs which is neceſſary for extricating one from diſagreeable ſituations. I have more than once experienced that it is poſſible to recover this coolneſs by reſolving to look only forwards, and endeavouring, by an effort of the imagination,

to

to ſuppoſe that you are walking merely on the highway, or ſtepping from one ſtone to another, or along the top of a ſtone fence in order to avoid wetting your feet.

In a word, it is neceſſary to place the moſt implicit confidence in the cautions of your guide. Men of that claſs, when they accompany a traveller, lie under the weight of great reſponſibility. If an accident ſhould take place through their fault, it would ruin their reputation; and on this reputation their maintenance depends. The guides, therefore, are intereſted by one of thoſe motives which are deareſt to man, to give good advice; and the traveller ought to obey it. From theſe precautions, which may be called moral, I ſhall now proceed to the phyſical.

The moſt eſſential of all is proper ſhoes. Three kinds of difficult declivities are found on the Alps—rocks, declivities of ice, and thoſe covered with graſs, which are more ſlippery than ice when the ſole of the ſhoe is ſmooth, as it generally becomes by walking over them. The uſe of cramp-irons either on the heel, as Sauſſure recommends, or placed acroſs the ſole, as they are uſed by ſome of the mountaineers, will render travelling over theſe declivities ſecure to a certain degree: but they muſt often be put on and taken off, becauſe they render walking more or leſs cumberſome; which is a great inconvenience. I have found ſo much utility in an invention which I ſubſtitute for them, that I do not heſitate to recommend it to thoſe fond of viſiting theſe romantic ſcenes.

The traveller muſt provide ſtrong ſhoes, the ſoles of which are at leaſt ſix lines in thickneſs, and having the upper leather and quarter double to a certain height around the ſole. The upper leather muſt be exceedingly pliable, in order that it may nowhere hurt the foot, and the ſhoes ſhould be tried ſeveral times in ſhort excurſions before a long journey is undertaken with them. Nails of tempered ſteel muſt be provided, the tails of which form a ſcrew; their heads, not leſs than four lines and a half in diameter, muſt be cut into the form of a ſquare pyramid, which will have two points in conſequence of the notch cut into each for receiving the ſcrew-driver to fix them in the ſhoes. Twelve of theſe nails

muſt be put into each ſole, *viz.* ſeven around the fore-part, diſpoſed at equal diſtances, and five around the heel, as near the edge of the ſole as poſſible without endangering the burſting of the leather. The interval between theſe nails ought to be filled up with common large-headed nails, ſo cloſe that their heads may touch each other.

Shoes of this kind will enſure ſafety to the traveller in all places of difficulty; the nails will hold on granite as well as on the graſs, will occaſion no inconvenience in the plains, and will laſt for a long time. When the ſteel heads become worn down, others may be ſubſtituted in their place. The firſt ſhoes of this kind, which I cauſed to be made a dozen years ago, and which I have frequently employed during that period, are ſtill fit for ſervice.

A ſtaff, five or ſix feet in length, ſhod with iron, is alſo uſeful to thoſe who traverſe the glaciers, either for ſounding the deceitful ſnow which covers the fiſſures, or for walking with more ſafety over the ſlippery ice. Staves of this kind, ready prepared by the guides, may be found at Chamouni.

When the traveller has to aſcend for a conſiderable time, it is a bad plan to be deſirous of hurrying. By theſe means the traveller becomes heated, and loſes breath, and his progreſs is more retarded than he expected. The guide muſt be ordered to walk foremoſt; and it will be neceſſary to be directed by his ſteps, which are meaſured, as it were, in cadence, and to ſtop from time to time, but without ſitting down, and without halting too long to catch cold. He may aſcend in this manner, without ſtopping, 200 fathoms perpendicular height, and that is enough. He muſt pull off his coat when he begins to perſpire, and carry it folded up on his ſhoulder: in this manner the heat produced by the labour of walking may be moderated, and he will find, when he halts, the benefit of an additional covering, without any of its inconveniences. In regard to dreſs, it is of eſſential advantage, when a traveller ſets out for the glaciers, whatever may be the heat in the plain country below, to take proper precautions to guard againſt the cold, which it is not always poſſible to foreſee. To the ladies in particular I recommend this attention.

Theſe

Theſe precepts are addreſſed, more or leſs directly, to all travellers who may be deſirous of traverſing the mountains; but I ſhall add a few words to thoſe who are philoſophers or naturaliſts, and to the amateurs of lithology in particular. The latter have frequently experienced the inconvenience of having nothing but their pockets to contain the ſpecimens of rock which they detach with a hammer: theſe are ſoon filled, and haraſs the traveller by their ſwinging movement. I ſupply this deficiency, with advantage, in the following manner:

To a pretty broad leather belt I have adapted a loop of the ſame material, placed in an inclined poſition, to receive my hammer, nearly in the ſame manner as the Turks carry their poniards. On the other ſide is a ſmall pocket, which contains a bottle of acid in a wooden caſe, a piece of ſteel for ſtriking fire, &c. This belt or girdle forms the upper part of an apron of very thin leather, which, when let down, would cover the knee, but which, when tucked up on each ſide, forms a large horizontal pocket before, open at the top, and ſupported in the middle by a thong ſhaped like an inverted Y; the two branches of which are ſewed to the girdle, and paſs round the apron below. The tail of the Y is turned up before, and is buckled to the bandolier with which I carry my barometer. The ſtones which I put into this pocket being diſpoſed, as it were, around the centre of gravity of the body, and being ſupported in part by the ſhoulders, on account of the thong which envelops the apron, occaſion no inconvenience. I thus have them all under my eye, and at hand when I wiſh to ſubſtitute one ſpecimen for another; and they experience no agitation, and do not rub againſt each other as they do in the pockets.

To the ſame girdle, on one ſide, are ſuſpended, by ſteel hooks, one of Ramſden's ſextants of three inches radius, which gives even the minutes of a degree; an inſtrument exceedingly convenient for meaſuring angles. On the other ſide is ſuſpended an artificial horizon, with a ſpirit level for taking heights. The box of this inſtrument I diſpoſe in ſuch a manner that it ſerves me for a ſtand when neceſſary, being ſupported by a ſtaff that opens at bottom with three

feet ſ

feet; it ſerves alſo as a ſupport for my barometer, and makes an excellent walking-ſtick when the three branches are folded together. Though the whole of theſe inſtruments, added to the uſual quantity of ſtones collected during an excurſion of this kind, amount in weight to more than twenty pounds, I am not much incommoded by the burden, on account of its being equally diſtributed; and in my laſt Alpine journey, which I undertook with the unfortunate Dolomieu, he envied my lot, though, inſtead of being accoutred as I was, he carried only his hammer and his ſpecimens.

IV. *A cheap and efficacious Method for deſtroying Rats and Mice; recommended to the Agriculture Society of Manchester* by Mr.* C. TAYLOR, *Secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce.*

IN or near the places frequented by theſe vermin, place upon a ſlate or tile one or two meat ſpoonfuls of dry oatmeal; lay it thin, and preſs it flat, that you may more eaſily know what is taken away. The rats, if not interrupted, will come regularly to feed there; ſupply them thus with freſh oatmeal for two or three days; then to about ſix ſpoonfuls of dry oatmeal add three drops of oil of aniſeeds; and having ſtirred the mixture well together, feed them with this for two or three days more; then for one day give them only half the quantity they have uſually eaten of this ſcented oatmeal, and on the following day place the following mixture:

To four ounces of dry oatmeal, ſcented with ſix drops of oil of aniſeeds, add half an ounce of carbonat of barytes, previouſly pounded very fine in a mortar, and ſifted through a little fine muſlin or cambric: mix this intimately with the ſcented oatmeal, and lay this mixture of oatmeal and barytes upon the tile or ſlate, as the oatmeal had been uſually placed, and allow the rats to come and eat of it for twenty-four hours without interruption.

* For this communication Mr. Taylor received the thanks of the Society. As that Society has not yet publiſhed any of its tranſactions, our inſerting this article cannot fail to prove highly uſeful.

A few

A few hours after eating thereof, you will frequently see some of them running about as if drunk, or paralytic, but eventually they generally all retire to their haunts and die. As rats are extremely sagacious, it may be proper, where they have only eaten a small portion, to allow the mixture to remain for forty-eight hours. It will be best to burn what is left after that time, as a fresh mixture may be prepared at a trifling expense, when wanted.

During the time in which the mixture of barytes is exposed to the rats, it is necessary to keep shut the doors of the places where it is laid, to prevent the vermin from being disturbed, or a possibility of accident to any other animal or person; for though it is not so extremely dangerous, if taken internally, as the preparations commonly employed for killing rats, and is even in some cases used in medicine, yet it is fatal if taken improperly.

The oil of aniseeds renders the mixture disagreeable to dogs and many other animals, but is, in small quantities, alluring to rats.

The carbonat of barytes may be procured in large quantities at the lead mines belonging to Sir Frank Standish, Bart. at Anglezark, near Chorley: the proper sort is tasteless, semi-transparent, and effervesces with acids: it is moderately hard and striated. It is frequently called aërated barytes (*terra ponderosa aërata*), and sometimes, by the miners, ponderous spar.

V. *A brief Examination of the received Doctrines respecting Heat or Caloric. By* Alexander Tilloch. *Read before the Askesian Society, December* 1799.

[Continued from Page 78.]

A Quantity of heat added to that already in water of a known temperature, which shall *sensibly increase* the volume of the compound, heat and water, only *one thousandth* part, is *sensible* heat; caloric not in combination, nor in chemical union—it is cognisable by our senses! But if enough be added *sensibly to increase the volume to one thousand times its*

its original bulk, then the added heat is latent; it is in chemical union; it is combined caloric! Becauſe now not cogniſable by our ſenſes? No. We ſee the volume of the ſteam much larger than that of the water; and the more heat we add, the greater is the increaſe of volume while water is left to receive it. Yet I am not to believe that the heat is cogniſable by any organ of ſenſe or external ſign! In other words, I am commanded to believe as true, a ſtatement which the evidence of my own ſenſes makes it impoſſible I can give credit to.

But, ſay the advocates for this doctrine, a ſubſtance ſo charged with caloric as to become vapour, will not indicate, by the thermometer, any further increaſe of temperature, though we continue to pour heat into the liquid that produces the vapour; therefore the extra heat muſt have become *latent* in the ſteam that has been generated. Nay, we can prove the fact; for this ſame latent heat may again be made *ſenſible* in the common proceſs of condenſation.

This mode of reaſoning appears to me to be more ſpecious than juſt. It is demanding that the common thermometer ſhould do in this caſe what it never does in any. This inſtrument never tells the quantity of heat paſſing into any body, even in thoſe caſes where heat is counted *ſenſible* or *free*; it only tells the comparative quantity paſſing into itſelf from the body in which it is immerſed, or with which it is in contact, to bring it into equilibrium with that body as to heat.

Boiling water, ſteam, the materials of which the thermometer is made, become each charged with heat *in proportion to their capacities*, and this whether the thermometer be in the water or in the ſteam. The thermometer kept in the ſteam will never riſe higher than 212°, "becauſe there the heat is *latent!*" Keep it in boiling water for a year, and it will not riſe higher than 212°; yet there the heat is *ſenſible!* Is this diſtinction reconcilable with common ſenſe? But the reſult may even be altered at pleaſure. It will not riſe higher than 212 in boiling water *under the common atmoſphere*:—in other words, if we wiſh to raiſe the heat higher, we muſt put a greater preſſure upon the water. Confine the water ſo

that none of it may eſcape, and the heat will riſe in it far above 212°. Confine the ſteam, and in it the heat will riſe juſt as high as in the confined water; yet in the one the heat exiſts in a different ſtate from what it does in the other!!—The reſult, we ſee, may be altered by mechanical contrivances: nay, ſtrictly ſpeaking, What is the effect produced by the atmoſphere but mere mechanical preſſure? Yet we are to believe that a change has been effected in the *phyſical properties* of one of the ſubſtances ſubjected to its *mechanical operation!*—If Nature had ſo conſtituted the atmoſphere as to have only half its preſent gravity, the point at which heat would become latent, as it is called, in ſteam would have been far below 212°. When water is made to boil in an exhauſted receiver at a lower temperature, have we done any thing but removed weight from its ſurface, and *vice verſa?*—Does the heat in the ſteam in theſe caſes paſs into a latent ſtate alſo? If it does, the effect is mechanical: if it does not, then the mere accident of the atmoſphere being of its preſent weight, has nothing to do with the boiling point happening to fall at 212°! But he that would ſay ſo would be counted mad.

At Munich, and other places equally elevated above the level of the ſea, that is, having a leſs weight of atmoſphere upon them, water, in open veſſels, boils at 209°. In a partially exhauſted receiver the ſame effect takes place: and yet the doctrine of latent heat is never conſidered as inconſiſtent with the fact; really for no other reaſon but becauſe a common thermometer cannot meaſure *ſpecific* heat.

To demand that a thermometer ſhould meaſure the quantity of heat poured into water to convert it into vapour, and to maintain it in that form; and to inſiſt, becauſe the inſtrument will not do this, that the heat muſt have changed its nature, and loſt its original character; is about as wiſe as it would be to demand, that a pint meaſure dipt into the ocean ſhould determine the quantity of water in the latter, and to inſiſt that, otherwiſe, the water on the outſide of the veſſel muſt have loſt its original character, and be different from that within: it is demanding that mercury, which, by its conſtitution, can at 212° only expand a certain quantity, compared with its own bulk in ſome given lower tempera-

ture, ſhould be able (without our making any comparative experiments to determine the point) to inform us how many times water will be increaſed its own bulk when we pour a greater quantity of heat into it!

The heat in the ſteam is as much ſenſible or free heat as it was before it paſſed into the ſteam, if theſe terms are to be applied to heat cogniſable by our ſenſes, and that may be meaſured comparatively. But *the ſteam is really its own thermometer*; and it indicates as truly the quantity of heat that has paſſed into a given quantity of water, as the thermometer does the quantity that paſſes into itſelf; aye, and by the ſame means too—the magnitude of its own volume.

Inſtead of ſuppoſing, in the caſe of ſteam, that heat has become latent, or been *changed*, would it not be more correct to aſcribe the phænomenon that led to this idea to another cauſe, *a change in the form of the water*, which, by its conſtitution, is forced to become vapour, under the *common preſſure* of the atmoſphere, *whenever a certain number of times its own bulk of heat is poured into it*. The quantity, after proper comparative experiments, could then be expreſſed in ſenſible terms, and would turn out to be *the whole bulk of the ſteam, minus the original volume of the water in the compound*.

If ſome ſuch method were followed, it appears to me extremely probable that we ſhould ſoon arrive at many truths reſpecting the operations of the univerſally diffuſed ſubſtance *heat*, which otherwiſe muſt eſcape us, though the facts that might lead to them are daily preſenting themſelves in almoſt every chemical proceſs. It would ſurely tend much to the advancement of ſcience, if *the bulk*, *maſs*, or *volume* of heat neceſſary to convert different ſolids into liquids, and liquids into gaſes, *under a given preſſure*, were accurately determined by experiment. The thermometer would then be a more uſeful inſtrument than it now is—But we ſhould never look to it to perform impoſſibilities; we ſhould no more expect it to meaſure the quantity of heat paſſing into or out of bodies, than we ſhould attempt to meaſure the quantity of water delivered from a pump, by placing an hygrometer or any twiſted fibrous ſubſtance in the ſtream, and then exa-

mining how much its length is diminiſhed or its diameter increaſed.

Some may ſay, that the caſe of the converſion of water into ſteam is not held by them as one of thoſe that prove the paſſing of heat from a ſenſible to a latent ſtate, and that therefore our reaſoning, drawn from that example, will not invalidate the doctrine; "for in the caſe of water they conſider the heat as in ſimple mixture, and it would be an abuſe of words to call ſo weak an union by the name of combination."

I reply, that this caſe was one of the earlieſt brought in ſupport of the doctrine, and alſo thought to be one of the ſtrongeſt; and if the good underſtanding of any has led them to give it up, it is the more ſurpriſing they ſhould be ſo blind as to continue upholding a fabric which was built on this as one of its foundation ſtones; and which does not appear to me to be upheld by any one fact that may not be as ſatisfactorily explained, without admitting the exiſtence of heat in two diſtinct ſtates.

But, ſay theſe, two different fluids of the ſame temperature when united will often give out heat—What can we ſay of this heat, but that it was *latent* or *combined* in one or both of the fluids, and that it is thrown out as ſenſible or free heat by their union? I would ſay no ſuch thing without a previous examination of all the accompanying phænomena; and one of theſe I find to be *a reduction in the volume of the compound*, which is leſs than that of the ſum of the two. The moleculæ of the two ſubſtances occupy leſs room united than in their reſpective fluids. For inſtance, when ſulphuric acid and water are joined, the volume of the mixture is leſs than that of the two before mixture; there is, therefore, leſs lodging-room left for the heat: in other words, the capacity of the compound for heat is leſs than the ſum of the capacities of the ingredients: therefore, compared with ſurrounding bodies, it has now too large a quantity, and, by the law of equilibrium, muſt give off the ſurplus to the ſurrounding bodies in proportion to their capacities, reſerving of ſuch ſurplus only that portion due to itſelf, and neceſſary to give it ſuch an increaſed temperature as the ſurrounding bodies

will each have acquired, by the diffufion of the diflodged heat, when it has come again to a ftate of equilibrium; a quantity which muft in general be fo fmall as to elude all meafurement in the petty proceffes of the laboratory.

Are there any cafes in which heat is diflodged by the union of two liquids, and where, at the fame time, the volume of the mixture is not reduced below that of the fum of the volumes of the ingredients? I do not recollect any. There may, however, be fome, and it will be time enough to attempt to explain them when they are adduced. In the mean time, when a diminution of volume follows, or rather accompanies, the extrication of heat from any body, inftead of running to the doctrine of latent heat being then made fenfible (that is, heat being changed in its character), we ought to content ourfelves with ftating an obvious fact, namely, that the moleculæ of the two liquids are fo conftructed and formed as to admit of their coming clofer together when mixed than they could when refpectively alone; and, of courfe, now fill reciprocally fpaces that, before their mixture, were filled with heat: the latter fubftance, in confequence of being thus diflodged (for two fubftances cannot, at one and the fame time, occupy the fame fpace), diffufes itfelf among the furrounding bodies in proportion to their capacities, conftituting in them, what it did in thofe it has quitted, *bulk* or *volume*.

What takes place in fuch cafes may be illuftrated by one of a different kind. If a pint of fmall fhot and a pint of dry fand be mixed, they will occupy a lefs volume than two pints; air is thrown out that was before lodged in the interftices of both of them. Was it *latent air* then, and is it *fenfible air* now? Did it differ in its properties before and after being ejected? Weighing the ingredients before and after mixture, will not tell how much air has been ejected: but we know, notwithftanding, that its volume may be meafured; and fo may that of the heat driven out in the cafe which this was brought to illuftrate.

How are fluids in general meafured but by their bulk? or their weight, when circumftances will allow it? Water, for inftance, in common cafes, is directly meafured by any veffel

veſſel whoſe capacity is known or can be come at; in others, as in a wet piece of wood, a brick, or other ſubſtance, the quantity muſt be found out by other means: but in every caſe where water is added to another ſubſtance, which is made thereby to expand exactly in the direct ratio of the quantity of water added (if there be any ſuch), the quantity may be determined by meaſuring the compound, and deducting therefrom the original volume of the other ſubſtance. *Would it be abſurd to talk of meaſuring the matter, fluid, or ſubſtance, called heat, in a ſimilar manner?*

When a thermometer is applied to any ſubſtance of a higher temperature than itſelf, it is, by the operation of the general law, ſoon brought into equilibrium with that ſubſtance as to heat; and we ſay, "it has riſen ſo many degrees." We are habituated to this mode of ſpeaking, and ſatisfy ourſelves, without any more inquiry, that the phænomenon requires no further inveſtigation; and as to the accompanying phænomena, we generally overlook them altogether. When we find that the mercury has increaſed in volume, would it be abſurd to aſk this ſimple queſtion? Is the increaſe to be attributed not merely to the addition of heat, but to the addition of a quantity *equal in bulk to the increaſe of volume acquired by the mercury?* I think it extremely probable that the amount of increaſe or diminution of the volume of any ſubſtance, when heat is added or abſtracted, is the real bulk of the heat ſo added or abſtracted. That I may be clearly underſtood, I ſhall illuſtrate my meaning by a compariſon:

If to a cubic inch of a compact piece of gum and water new unknown quantities of water be added, who would ever think of wire-drawing the mixture through a tube, and expreſſing the reſult in degrees of no known quantity, nor referable to any determinate meaſure? The caſes to me appear perfectly parallel.

To a mixture of gum and water we add water, and the volume of the mixture is increaſed; and to a mixture of mercury and heat we add heat, and the volume of that mixture is increaſed.

When

When the volume of a ſubſtance is increaſed by heat, can any thing be conceived more eaſily practicable, in many caſes, than to determine, by actual meaſurement, the proportion that the increaſe bears to the volume of the maſs at a given lower temperature? Is not this already done in many caſes? The experiments on this point ſhould be multiplied, ſo as to embrace, if poſſible, every known ſubſtance and every degree of heat. Moſt ſubſtances would then become their own thermometers: nay, all are ſo at preſent, but we have not examined the relations of all their different ſcales.

[To be continued.]

VI. *Experiments on the Solar and on the Terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame, or different.* By WILLIAM HERSCHEL, *LL.D. F.R.S.*

[Continued from Page 21.]

5th Experiment. Reflection of the Heat of a Coal Fire by a plain Mirror.

I PLACED a ſmall ſpeculum, ſuch as I uſe with my 7-feet reflectors, upon a ſtand, and ſo as to make an angle of 45 degrees with the front of it*. This was afterwards to face the fire in my parlour chimney, and would make the ſame angle with the bars of the grate. At a diſtance of $3\frac{1}{2}$ inches from the ſpeculum, on the reflecting ſide of it, was placed the thermometer No. 1; and cloſe by it, but out of the reach of the reflected rays, the thermometer No. 4. The whole was guarded in front, againſt the influence of the fire, by an oaken board $1\frac{1}{2}$ inch thick, which had a circular opening of $1\frac{1}{4}$ inch diameter, oppoſite the ſituation of the plain mirror, in order to permit the fire to ſhine upon it. The thermometers were divided from the mirror by a wooden partition, which alſo had an opening in it, that the reflected

* See Plate II. fig. 2.

rays

rays might come from the mirror to No. 1, while No. 4 remained ſcreened from their influence. On expoſing this apparatus to the fire, I had the following reſult:

	No. 1.	No. 4.	
0′	60	60	Here, in five minutes, the heat reflected from the plain mirror raiſed the thermometer No. 1, 7 degrees; while the change in the temperature of the ſcreened place, indicated by No. 4, amounted only to half a degree: which ſhows
1	62	60	
2	64	60	
3	66	60	
4	66	60	
5	67	60½	

that an open fire ſends out rays that are ſubject to the laws of reflection, and occaſion heat.

6th Experiment. Reflection of Fire-Heat by a Priſm.

Every thing remaining arranged as in the 5th experiment, I removed the ſmall plain mirror, and placed in its ſtead a priſm which had one of its angles of 90 degrees, and the other two of 45 degrees each *. It was put ſo as to have one of the ſides facing the fire, while the other was turned towards the thermometer: the hypotenuſe, conſequently, made an angle of 45 degrees with the bars of the grate. The apparatus, after having been cooled ſome time, was expoſed to the fire, and the following reſult was taken:

	No. 1.	No. 4.	
0′	62½	62½	Here, in eleven minutes, the rays reflected by the priſm raiſed the thermometer 4½ degrees; but, the temperature of the place having undergone an alteration of 1¾ degrees, we can only place 2¾ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully ſufficient to ſhow, that even a priſm, which ſtops a great many heat-making rays, ſtill reflects enough of them to prove, that an open fire not only ſends them out, but that they are ſubject to every law of reflection.
1	63	62¾	
2	64	63	
4	64½	63	
5	65	63¼	
8	65½	63½	
10	66½	63¾	
11	67	64¼	

* See Plate II. fig. 2. F.

7th Experiment. Reflection of invisible Solar Heat.

On a board of about 4 feet 6 inches long, I placed at one end a small plain mirror, and at the other two thermometers *. The distance of No. 1, from the face of the mirror, was 3 feet 9½ inches; and No. 2 was put at the side of it, facing the same way, but out of the reach of the rays that were to be reflected by the mirror. The colours of the prism were thrown on a sheet of paper, having parallel lines drawn upon it, at half an inch from each other. The mirror was stationed upon the paper; and was adjusted in such a manner as to present its polished surface, in an angle of 45 degrees, to the incident coloured rays, by which means they would be reflected towards the ball of the thermometer No. 1. In this arrangement, the whole apparatus might be withdrawn from the colours to any required distance, by attending to the last visible red colour, as it showed itself on the lines of the paper. When the thermometers were properly settled to the temperature of their situation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours, so far as to cause the mirror, which was now open, to receive only the invisible rays of heat which lie beyond the confines of red. The result was as follows:

	No. 1.	No. 2.	
0′	56	56	Here, in ten minutes, the thermometer No. 1 received four degrees of heat, reflected to it, in the strictest optical manner, by the plain mirror of a Newtonian telescope. The great regularity with which these invisible rays obeyed the law of reflection was such, that Dr. Wilson's sensible thermometer, No. 2, which had been chosen on purpose for a standard, and was within an inch of the other thermometer, remained all the time without the least indication of any change of temperature that might have arisen from straggling rays, had there been any such. I now took away the mirror, but left every thing else in the situation it was. The effect of this was thus:
—	57	56	
—	59	56	
7	60	56	
10	60	56	

* See Plate IV. fig. 1.

No. 1,

	No. 1.	No. 2.	
0′	60	56	Here, in ten minutes, the thermometer No. 1 loſt again the four degrees it had acquired, while No. 2 ſtill remained unaltered; and this becomes therefore a moſt deciſive experiment, in proof of the exiſtence of inviſible rays, of their being ſubject to the laws of reflection, and of their power of occaſioning heat.
5	58	56	
8	57	56	
10	56	56	

8th Experiment. Reflection and Condenſation of the inviſible Solar Rays.

I made an apparatus for placing the ſmall ſteel mirror at any required angle*; and, having expoſed it to the priſmatic ſpectrum, ſo as to receive it perpendicularly, I cauſed the colours to fall on one half of the mirror, which, being covered by a ſemicircular piece of paſteboard, would ſtop all viſible rays, ſo that none of them could reach the poliſhed ſurface. On the paſteboard were drawn ſeveral lines, parallel to the diameter, and at the diſtance of one-tenth of an inch from each other; that, by withdrawing the apparatus, I might have it at option to remove the laſt viſible red to any required diſtance from the reflecting ſurface. In the focus of the mirror was placed the thermometer No. 2. I covered now alſo the other half of the mirror, till the thermometer had aſſumed the temperature of its ſituation. Then, withdrawing the apparatus out of the viſible ſpectrum, till the laſt tinge of red was one-tenth of an inch removed from the edge of the paſteboard, and the whole of the coloured image thus thrown on the ſemicircular cover, I opened the other half of the mirror for the admiſſion of inviſible rays. The reſult was as follows:

	No. 2. In the Focus of inviſible Heat.	
0′	61	Here, in one minute, the thermometer roſe 19 degrees. I covered the mirror.
1	80	
2′	72	Here, in three minutes, the thermometer fell 16 degrees. I opened the mirror again.
3	67	
4	64	

* See Plate II. fig. 1.

	No. 2. In the Focus of invisible Heat.	
5′	83	Here, in two minutes, the thermometer rose 24 degrees. I covered the mirror once more.
6	88	
7′	69—And, in one minute, the thermometer fell	

19 degrees. Now, by this alternate rising and falling of the thermometer, three points are clearly ascertained. The first is, that there are invisible rays of the sun. The second, that these rays are not only reflexible, in the manner which has been proved in the foregoing experiment, but that, by the strict laws of reflection, they are capable of being condensed. And, in the third place, that by condensation their heating power is proportionally increased; for, under the circumstances of the experiment, we find that it extended so far as to be able to raise the thermometer, in two minutes, no less than 24 degrees.

9th Experiment. Reflection of invisible Culinary Heat.

I planted my little steel mirror upon a small board*, and, at a proper distance opposite to it, I erected a slip of deal, $\frac{1}{2}$ inch thick and 1 inch broad, in a horizontal direction, so as to be of an equal height, in the middle of its thickness, with the centre of the mirror. Against the side, facing the mirror, were fixed the two thermometers No. 2 and No. 3, with their balls within half an inch of each other, and the scales turned the opposite way. A little of the wood was cut out of the slip, to make room for the balls to be freely exposed. That of No. 2 was in the axis of the mirror, and the ball of No. 3 was screened from the reflected rays by a small piece of pasteboard tied to the scale. The small ivory scales of the thermometers, with the slip of wood at their back, which, however, was feather-edged towards the stove, intercepted some heat; but it will be seen presently that there was enough to spare. When my stove was of a good heat, I brought the apparatus to a place ready prepared for it.

	No. 2. In the Focus.	No. 3. Screened.	
6′	52	52	Here we find that, in one minute, the invisible culinary heat raised the thermometer
7	91	53	No. 2, 39 degrees; while

* See Plate IV. fig. 2.

No. 3,

No. 3, from change of temperature, roſe only one, notwithſtanding its expoſure to the ſtove was in every reſpect equal to that of No. 3, except ſo far as relates to the rays returned by the mirror; and therefore the radiant nature of theſe inviſible rays, their power of heating bodies, and their being ſubject to the laws of reflection, are equally eſtabliſhed by this experiment.

10th Experiment. Reflection of the inviſible Rays of Heat of a Poker, cooled from being red-hot till it could no longer be ſeen in a dark Place.

The great abundance of heat in my laſt experiment would not allow of its being carried on without injury to the thermometer, the ſcale of which is not extenſive; I therefore placed a poker, when of a proper black heat, at 12 inches from the ſteel mirror *, and received the effect of its condenſed rays upon the thermometer No. 2, placed in the focus. Then, alternately covering and uncovering the mirror, one minute at a time, the effect was as follows:

		No. 2.	
The mirror covered	0′	61	Here, in ſix minutes, we have a repeated reſult of alternate elevations and depreſſions of the thermometer, all of which confirm the reflexibility, the radiant nature, and the heating power of the inviſible rays that came from the poker.
Open -	1	68	
Covered -	2	61	
Open -	3	64	
Covered -	4	59	
Open -	5	61½	
Covered -	6	58	

From theſe experiments it is now ſufficiently evident, that in every ſuppoſed caſe of ſolar and terreſtrial heat, we have traced out rays that are ſubject to the regular laws of reflection, and are inveſted with a power of heating bodies; and this independently of light. For, though, in four caſes out of ſix, we had illuminating as well as heating rays, it is to be noticed that our proof goes only to the power of occaſioning heat, which has been ſtrictly aſcertained by the thermometer. If it ſhould be ſaid, that the power of illuminating objects of theſe ſame rays is as ſtrictly proved by the ſame experiments,

* See Plate I. fig. 4.

I muſt remark that, from the caſes of inviſible rays brought forward in the four laſt experiments, it is evident that the concluſion that rays muſt have illuminating power becauſe they have a power of occaſioning heat is erroneous; and, as this muſt be admitted, we have a right to aſk for ſome proof of the aſſertion, that rays which occaſion heat can ever become viſible. But, as we ſhall have an opportunity to ſay more of this hereafter, I proceed now to inveſtigate the refraction of heat-making rays.

11th Experiment. Refraction of Solar Heat.

With a new ten-feet Newtonian teleſcope, the mirror of which is 24 inches in diameter of poliſhed ſurface, I received the rays of the ſun; and, making them paſs through a day-piece with four lenſes, I cauſed them to fall on the ball of the thermometer No. 3, placed in their focus. Thoſe who are acquainted with the lines in which the principal rays and pencils move through a ſet of glaſſes, will eaſily conceive how artfully, in our preſent inſtance, heat was ſent from one place to another—heat croſſing heat, through many interſecting courſes, without joſtling together, and each parcel arriving at laſt ſafely to its deſtined place. As ſoon as the rays were brought to the thermometer, it roſe, almoſt inſtantly, from 60 degrees to 130; and, being afraid of cracking the glaſſes, I turned away the teleſcope. Here the rays, which occaſioned no leſs than 70 degrees of heat, had undergone eight regular ſucceſſive refractions; ſo that their being ſubject to its laws cannot be doubted.

12th Experiment. Refraction of the Heat of a Candle.

I placed a lens of about 1,4 inch focus and 1,1 inch diameter, mounted upon a ſmall ſupport, at a diſtance of 2,8 inches from a candle*; and the thermometer No. 2, behind the lens, at an equal diſtance of about 2,8 inches; but which ought to be very carefully adjuſted to the ſecondary focus of the candle. Not far from the lens, towards the candle, was a paſteboard ſcreen, with an aperture of nearly the ſame ſize as the lens. The ſupport of the lens had an

* See Plate IV. fig. 3.

eccentric

eccentric pivot, on which it might be turned away from its place, and returned to the ſame ſituation again, at pleaſure. This arrangement being made, the thermometer was for a few moments expoſed to the rays of the candle, till it had aſſumed the temperature of its ſituation. Then the lens was turned on its pivot ſo as to intercept the direct rays which paſſed through the opening in the paſteboard ſcreen, and to refract them to the focus, in which the thermometer was ſituated.

	No. 2.	
0′	53⅞	Here, in three minutes, the thermometer received 2½ degrees of heat, by the refraction of the lens. The lens was now turned away.
1	55½	
2	55¾	
3	56	
0′	56	Here, in three minutes, the thermometer loſt 2⅛ degrees of heat. The lens was now returned to its ſituation.
1	54⅝	
2	54⅛	
3	53⅞	
0′	53⅞	And, in three minutes, the thermometer regained the 2⅛ degrees of heat. A greater effect may be obtained by a different arrangement of the diſtances. Thus, if the
1	54¾	
2	55⅝	
3	56	

lens be placed at 3½ inches from a wax candle, and the thermometer ſituated, as before, in the ſecondary focus, we ſhall be able to draw from 5 to 8 degrees of heat, according to the burning of the candle, and the accuracy of the adjuſtment of the thermometer to the focus. The experiment we have related ſhows evidently that rays inveſted with a power of heating bodies iſſue from a candle, and are ſubject to laws of refraction, nearly the ſame with thoſe reſpecting light.

13*th Experiment. Refraction of the Heat that accompanies the coloured Part of the Priſmatic Spectrum.*

I covered a burning lens of Mr. Dollond's, which is nearly 9 inches in diameter, and very highly poliſhed, with a piece of paſteboard, in which there was an opening of a ſufficient ſize to admit all the coloured part of the priſmatic ſpectrum *. In the focus of the glaſs was placed the thermometer No. 3; and, when every thing was arranged properly, I covered the

* See Plate IV. fig. 4.

lens

lens for five minutes, that the thermometer might assume the temperature of its situation. The result was as follows:

		No. 3.	Here, in one minute, the thermometer received 112 degrees of heat,
The lens covered	0′	64	
Open -	1	176	

which came with the coloured part of the solar spectrum, and were refracted to a focus; so that, if the coloured rays themselves are not of a heat-making nature, they are at least accompanied with rays that have a power of heating bodies, and are subject to certain laws of refraction, which cannot differ much from those affecting light.

[To be continued.]

VII. *An Examination of* St. Pierre's *Hypothesis respecting the Cause of the Tides, which, in opposition to the received Theory, attributes them to supposed periodical Effusions of the Polar Ices. By* Samuel Woods, *Esq. Read before the Askesian Society November* 5, 1799.

THE tides are two periodical motions actuating the ocean (called the flux and reflux, or ebb and flow), which succeed each other alternately at an interval of about six hours; the period of a flux and reflux being, upon an average, 12 hours 24 minutes, the double of which, 24 hours 48 minutes, corresponds to that of a lunar day, or the time elapsing between the moon's passing a meridian and coming to it again. These alternate elevations and depressions of the ocean so exactly correspond with the course of the sun and moon, as to time and quantity, that the influence of those luminaries has in all ages been considered as the cause of their production; but it was reserved for modern times to ascertain the principle of their laws, and to calculate, with precision, the effects produced by the different situations of the sun and moon, and the proportions of their power. This principle is no other than gravitation. It is evident that, if the earth were entirely fluid and quiescent, its particles, by their mutual gravity, would form the whole mass into a perfect sphere: now, if any power be supposed to act on all the particles of this sphere

with equal force, and in parallel directions, the whole mass would be moved together without experiencing any alteration in its figure. But this is not the case with respect to the moon's action on our globe: the power of gravity diminishes as the square of the distance increases, and therefore the waters (at Z, Plate V. fig. 1.) on the side of the earth (A,B,C,D) next the moon (M), are more attracted by the moon than the central parts of the earth (O), and the central parts more attracted than the waters on the opposite side (at n); and therefore the distance between the earth's centre and the waters on its surface under and opposite to the moon will be increased. For, suppose three bodies in the same line (H,O,D), if they are all equally attracted by any power (as M), they will all move towards it with equal rapidity, their mutual distances continuing the same; but if the attraction of this power (M) is unequal, the body most forcibly attracted will move fastest, and their reciprocal distances will be proportionally increased: thus, the power of gravitation acting unequally on the three bodies (H,O,D), the distance of the first (H) from the second (O), and of the second from the third (D), will be increased in proportion to the difference of the gravitating power at the distance of the three bodies (H,O,D) respectively: now, suppose a number of bodies (ABCD) placed round the centre (O) so as to form a fluid ring, unequally attracted by some power (M), the parts nearest and furthest (H and D) from this power will have their distance from the centre (O) increased, while the sides of this ring (B and F), being nearly equidistant from the power (M), the centre (O) will not recede, but rather approach the centre (O). and form an ellipsis (nLIzN). To apply this reasoning to the case under consideration, while the earth, by its gravity, tends toward the moon, the water directly below her will swell and rise gradually; the water on the opposite side will recede from the centre (or, more properly, the centre will advance), and rise, or appear to rise, while the water at the sides is depressed, and falls below the former level: hence, as the earth revolves on its axis from the moon to the moon again in 24 hours 50 minutes, there will be two tides of ebb and two of flood in that period. In consequence

ſequence of the earth's motion on her axis, the moſt elevated part of the water is carried beyond the moon in the direction of the rotation, and continues to riſe after it has paſſed directly under the moon, not attaining its greateſt elevation till it has got about half a quadrant further. It continues alſo to deſcend, after it has paſſed at 90° diſtance from the point below the moon, to a like diſtance of about half a quadrant; and therefore in open ſeas, where the water flows freely, the time of high water does not exactly coincide with the time of the moon's coming to the meridian, but is ſome time after. Beſides, the tides do not always anſwer to the ſame diſtance of the moon from the meridian, ſince they are variouſly affected by the ſun's action, which brings them on ſooner when the moon is in her firſt and third quarters, and keeps them back later when ſhe is in her ſecond and fourth: becauſe, in the former caſe, the tide raiſed by the ſun alone would be earlier than the tide raiſed by the moon: in the latter caſe, later.

We have hitherto conſidered the moon as the principal agent in producing tides, but it is obvious that the inequality of the ſun's action muſt produce a ſimilar effect; ſo that, in reality, there are two tides every natural day occaſioned by the ſun, as well as two tides every lunar day occaſioned by the moon, and ſubject to the ſame laws: on account, however, of the ſun's immenſe diſtance, his action is conſiderably inferior to that of the moon. By comparing the ſpring and neap tides at the mouth of the Avon, below Briſtol, Sir Iſaac Newton calculates the proportion of the moon's force to the ſun's as 9 to 2 nearly. Dr. Horſley, in his edition of the Principia, eſtimates it as 5,0469 to 1; and, conſidering the elevation of the waters by this force as an effect ſimilar to the elevation of the equatorial above the polar parts of the earth, it will be found that the moon is capable of producing an elevation of about ten feet, the ſun of about two feet; which correſponds pretty nearly to experience.

In order to underſtand the cauſe of ſpring and neap tides, we muſt conſider, that the moon, revolving round the earth in an elliptic orbit, approaches nearer and recedes further from it, than her mean diſtance, in every revolution or lunar month. When neareſt, her attraction is ſtrongeſt, and *vice verſâ*:

verſi: when both luminaries are in the equator, and the moon in perigeo, the tides riſe higheſt, particularly at oppoſition and conjunction: at the change, when the attractive forces of the ſun and moon are combined, the tide is raiſed to a greater height: at the full, when the moon raiſes the tide under and oppoſite to her, the ſun, acting in the ſame line, raiſes the tides under and oppoſite to him, whence their conjunct effect is the ſame as at the change, and in both caſes occaſions what we call ſpring tides: but at the quarters, the ſun's action diminiſhes the effect of the moon's action, ſo that they riſe a little under and oppoſite the ſun, and fall as much under and oppoſite the moon, theſe two luminaries acting obliquely on each other, and producing what is called neap tides *.

The ſpring tides, however, do not happen preciſely at the full and change of the moon, nor the neap tides at the quarters, but about two days later. In this, as in many other caſes, the effects are not greateſt, or leaſt, when the immediate influence of the cauſe is greateſt or leaſt: as, for inſtance, the greateſt heat of ſummer is not at the time of the ſolſtice, but ſome weeks after; and if the actions of the ſun and moon ſhould be ſuddenly ſuſpended, the tides would continue for ſome time in their uſual courſe. The variations of the moon's diſtance from the earth produce a ſenſible difference in the tides. When the moon approaches the earth, her action on every part increaſes, and the differences of her action increaſe in a higher proportion as the moon's diſtances decreaſe. According to Sir Iſaac Newton, the tides increaſe as the cubes of the diſtances decreaſe; ſo that the moon, at half her diſtance, would produce tides eight times as great. The ſun being nearer the earth in winter than in ſummer, the ſpring tides are higheſt, and the neap tides loweſt, about the time of the equinoxes, a little after the autumnal and before the vernal; and, on the contrary, the ſpring tides loweſt and the neap tides higheſt at the ſolſtices, when the

* In Fig. 2. Plate V. HZON repreſents the earth; ABCD the moon's orbit. At the full and change, the ſun and moon act in the ſame line SP: at the quarters, the ſun's influence in the line SOH counteracts that of the moon acting in the direction MZN, and produces neap tides.

ſun is moſt diſtant from the equator. When the moon is in the equator, the tides are equally high in both parts of the lunar day; but as the moon declines towards either pole, the tides are alternately higher and lower at places having north or ſouth latitude: while the ſun is in the northern ſigns, the greater of the two diurnal tides in our climates is that ariſing from the moon above the horizon: when the ſun is in the ſouthern ſigns, the greateſt is that ariſing from the moon below the horizon. Thus the evening tides in ſummer are obſerved to exceed the morning tides, and in winter the morning tides exceed the evening tides: the difference at Briſtol is found to be 15 inches, at Plymouth 12. It would be ſtill greater, but that a fluid always retains an impreſſed motion for ſome time, and conſequently the preceding tides always affect thoſe that follow.

If the earth were covered all over with the ſea to a great depth, the tides would be regularly ſubſervient to theſe laws; but various cauſes combine to produce a great diverſity of effect, according to the peculiar ſituation and circumſtances of places, ſhoals, fords, and ſtraits: thus, a ſlow and imperceptible motion of a large body of water, ſuppoſe two miles deep, will be ſufficient to elevate its ſurface ten or twelve feet in a tide's time; whereas, if the ſame quantity of water is forced through a narrow channel forty or fifty fathoms deep, it produces a very rapid ſtream, and of courſe the tide is found to ſet ſtrongeſt in thoſe places where the ſea grows narroweſt, the ſame quantity of water being conſtrained to paſs through a ſmaller paſſage, as in the ſtraits between Portland and Cape la Hogue in Normandy; and it would be ſtill more ſo between Dover and Calais, if the tide coming round the iſland did not check it.

The ſhoalneſs of the ſea and the intercurrent continents are the reaſons why the tides in the open ocean riſe but to very inconſiderable heights, when compared to what they do in wide-mouthed rivers opening in the direction of the ſtream of the tide; and that high water is ſome hours after the moon's appulſe to the meridian, as it is obſerved upon all the weſtern coaſt of Europe and Africa from Ireland to the Cape of Good Hope; in all which a ſouth-weſt moon makes high

high water; and the ſame is ſaid to be the caſe on the weſtern coaſt of America: ſo that tides happen to different places at all diſtances of the moon from the meridian, and conſequently at all hours of the day.

To allow the tides their full motion, the ſpace in which they are produced ought to extend from eaſt to weſt 90° at leaſt; ſuch being the diſtance between the places moſt raiſed and depreſſed by the moon's influence. Hence it appears that ſuch tides can only be produced in large oceans, and why thoſe of the Pacific exceed thoſe of the Atlantic ocean: hence alſo it is obvious why the tides in the torrid zone between Africa and America, where the ocean is narrower, are exceeded by thoſe of the temperate zones on either ſide: and hence we may comprehend why the tides are ſo ſmall in iſlands at a great diſtance from the ſhores, ſince the water cannot riſe on one ſhore without deſcending on the other: ſo that at the intermediate iſlands it muſt continue at a mean height between its elevations on thoſe ſhores.

The tide produced on the weſtern coaſt of Europe correſponds to this theory. Thus, it is high water on the weſtern coaſts of Ireland, Spain, and Portugal, about the third hour after the moon has paſſed the meridian; from thence it flows into the adjacent channels, as it finds the eaſieſt paſſage. One current, for example, runs up by the ſouth of England, and another by the north of Scotland; taking conſiderable time to move all this way, and occaſioning high water ſooner in the places at which it firſt arrives, and begins to fall at theſe places while the current is proceeding to others further diſtant in its courſe. On its return it is unable to raiſe a tide, becauſe the water runs faſter off than it returns, till, by the propagation of a new tide from the ocean, the current is ſtopt, and begins to riſe again. The tide propagated by the moon in the German ocean, when ſhe is three hours paſt the meridian, takes twelve hours to come from thence to London bridge; ſo that, when it is high water there, a new tide has already attained its height in the ocean, and in ſome intermediate place it muſt be low water at the ſame time. When the tide runs over ſhoals, and flows upon flat ſhores, the water

is elevated to a greater height than in open and deep oceans that have steep banks, because the force of its motion is not broken upon level shores till the water has attained a greater height. If a place communicates with two oceans, or by two different openings with the same ocean, one of which affords an easier and readier passage than the other, two tides may arrive at this place in different times, which, interfering together, may produce a great variety of phænomena.

At several places it is high water three hours before the moon comes to the meridian; but that tide which the moon drives, as it were, before her, is only the tide opposite to that produced by her when nine hours past the opposite meridian.

It would be tedious to enumerate all the particular solutions easily deducible from these doctrines: as, why lakes and seas, such as the Caspian and the Mediterranean, the Euxine and the Baltic, have little or no sensible tides; since, having no communication, or being connected by very narrow inlets with the great ocean, they cannot receive or discharge water sufficient to alter their surface sensibly. In general, when the time of high water at any place is mentioned, it is to be understood on the days of new and full moon: the times of high water in any place fall at nearly the same hours after a period of about fifteen days, or between one spring tide and another.

This theory, however, is not without objections and difficulties; which has encouraged a Frenchman of some eminence, St. Pierre, to frame a new and singular hypothesis, ascribing all the phænomena of the tides to the periodical effusions of the polar ices. I shall first mention the most material facts and considerations which appear to militate against the common theory, as stated by St. Pierre; and I shall then endeavour to explain the theory he has substituted (which it has cost me some pains to collect, abstract, and arrange), as nearly as possible in a literal translation of his own language.

IT is said that, if the moon acted by her attraction, her influence must extend to the Mediterranean, the Baltic, the Cas-

pian, and the vaſt lakes of North America, in ſome degree at leaſt; but all theſe have no ſenſible tides*. This tranquillity renders her attraction liable to ſuſpicion; and we ſhall, perhaps, find that the greateſt part of the tides in the ocean have nothing more than an apparent relation either to her influence or her courſe.

The phaſes of the moon do not correſpond all over the globe with the movements of the ſeas. On our coaſts the flux and reflux follow the moon rather than her real motion: in various places they are ſubject to different laws, which obliged Newton to admit (chap. 25.) "that in the periodical return of the tides there muſt be ſome other mixed cauſe, hitherto undiſcovered."

The currents and tides in the vicinity of the polar circle come from the pole, as appears from the teſtimony of Fred. Martens, who aſſerts, that the currents amidſt the ices ſet in towards the ſouth; but adds, that he can ſtate nothing with certainty reſpecting the flux and reflux of the tides.—*Voyage towards the North Pole*, 1671.

Henry Ellis obſerved that the tides in Hudſon's bay came from the north, and were accelerated as the latitude increaſed. It is impoſſible theſe tides ſhould come from the line or the Atlantic. He aſcribes them to a pretended communication with the South ſea. At Waigat's ſtraits theſe north tides run at the rate of eight or ten leagues an hour. He compares them to the ſluice of a mill.—*Voyage to Hudſon's Bay*, 1746.

Linſcotten, in 1594, made nearly the ſame remarks, and obſerves that in Waigat's ſtraits the water was only brackiſh. He ſays the tides come from the eaſt with great velocity, bringing with them large iſlands of ice.

W. Barents (1595) confirms this account.

All theſe effects can be produced by nothing elſe than the effuſion of ices ſurrounding the pole. Theſe ices, which melt and flow with ſuch rapidity in the northern parts of America and Europe about the months of July and Auguſt,

* The Caſpian ſea is about 860 miles long, and, in one place, 260 miles broad: there are ſtrong currents, but no tides.

There is no regular flux and reflux in the Baltic.

In ſome particular ſpots of the Mediterranean there is a ſmall tide.

greatly

greatly contribute to our high equinoctial tides; and when theſe effuſions ceaſe in October, our tides begin to diminiſh.

If the tides depended on the action of the ſun and moon on the equator, they ought to be much more conſiderable towards the focus of their movements than any where elſe. But this is contrary to fact (Dampier ſays). From Cape Blanc, from the third to 30° ſouth lat. the flux and reflux of the ſea does not exceed two feet. The tides in the Eaſt Indies riſe not above a foot; near the poles they riſe 20 or 25 feet.

In the road of the iſland Maſſafuero (33° 46′ ſouth lat. 80° 22′ weſt long.) the ſea runs twelve hours north, and then flows back twelve hours ſouth: its tides, therefore, run towards the line.—*Byron, April* 1765.

At Engliſh Creek, on the coaſt of New Britain (5° ſouth lat. 152° weſt long.) the tide has a flux and reflux once in 24 hours.—*Carteret, Aug.* 1767.

At the Bay of Iſles, in New Zealand (35° ſouth lat.), the tides ſet in from the ſouth.—*Cook, Dec.* 1769.

At Endeavour River, in New Holland, neither the flood nor ebb tides were conſiderable, excepting once in 24 hours.—*June*, 1770.

At Chriſtmas Harbour, in Kergaelen's Land, the flood came from the ſouth-eaſt, running two knots an hour.—*Cook, Dec.* 1776. It appears to have been regular and diurnal, *i. e.* a tide of twelve hours. The tide riſes and falls about four feet.

At Otaheite the tides ſeldom riſe more than twelve or fourteen inches; and it is high water nearly at noon, as well at the quarters as at the full and change of the moon.—*Cook, Dec.* 1777. It is evident, from a table of theſe tides for 26 days, that there was but one tide a day; and this, during the whole time, was at its mean height between 11 and 1. Theſe tides, therefore, can have no relation to the phaſes of the moon.

Let us now take a curſory view of the effects produced by the tides in the northern part of the South ſea. At the entrance of Nootka it is high water, on the days of new and full moon, at twenty minutes paſt twelve: the perpendicular riſe and fall eight feet nine inches; which is to be underſtood of the

the day tides, and thoſe which happen two or three days after the full and change. The night tides riſe nearly two feet higher.—*Cook, April* 1778. Theſe ſemidiurnal tides differ from ours in taking place at the ſame hour, and exhibiting no ſenſible riſe till the ſecond or third day after the full moon: all which is perfectly inexplicable on the lunar hypotheſis.

Theſe northern tides of the South ſea, remarked in April, become, in higher latitudes, ſtronger in May, and ſtill ſtronger in June; which cannot be referred to the moon's courſe then paſſing into the ſouthern hemiſphere, but muſt be aſcribed to the ſun's courſe paſſing into the northern hemiſphere, and proceeding, as its heat increaſes, to fuſe the ices of the north pole: beſides, the direction of theſe northern tides towards the line conſtitutes a complete confirmation that they derive their origin from the pole.

At the entrance of Cook's River there was a ſtrong tide ſetting out of the inlet at the rate of three or four knots an hour: higher up in the inlet, at a place four leagues broad, the tide ran with prodigious violence at the rate of five knots an hour. Here the marks of a river diſplayed themſelves, the water proving conſiderably freſher.—*Cook, May* 1778.

What Cook calls a river, is nothing but a real northern ſluice, through which the polar effuſions are diſcharged into the ocean. Middleton (*Voyage to Hudſon's Bay*, 1741 and 1742) found, between lat. 65° and 66°, a conſiderable inlet running weſt, which he calls Wager's River; and, after repeated trials of the tides for three weeks, found the flood conſtantly coming from the eaſt. This is another of the northern ſluices.

In Karakakooa Bay, Sandwich Iſlands, the tides are very regular, ebbing and flowing ſix hours each alternately.—*Clerke, March* 1779.

At the town of St. Peter and Paul, in Kamſchatka, the tides are very regular every twelve hours.—*Clerke, Oct.* 1779.

Mr. Wales (*Introduction to Cook's laſt Voyage*) acknowledges that the tides obſerved in the middle of the great Pacific ocean fall ſhort full two-thirds of what might have been expected from calculation.

The courſe of the tides towards the equator in the South ſea;

sea; their retardations and accelerations on these shores; their directions, sometimes eastward, sometimes westward, according to the monsoons; finally, their elevation, which increases in proportion as we approach the poles, and diminishes in proportion to the distances from it, even between the tropics, demonstrate that their focus is not under the line. The cause of their motions depends not on the attraction or pressure of the sun and moon on that part of the ocean, for their forces would undoubtedly act there with the greatest energy, and in periods as regular as the course of the two luminaries.

Why, then, are the tides between the tropics so feeble and so much retarded under the direct influence of the moon?

Why does the moon, by her attraction, give us two tides every 24 hours in the Atlantic ocean, and produce only one in many parts of the South sea, which is incomparably broader?

Why do the tides take place there constantly at the same hours, and rise to a regular height almost all the year round?

Why do some rise at the quarters just the same as at the full and change?

Why are they always stronger as you approach the poles, and frequently set in toward the line, contrary to the principle of lunar impulsion?

These problems, which it is impossible to explain by the lunar theory, admit an easy solution on the hypothesis of the alternate fusion of the polar ices.——

Such are the most material objections adduced to invalidate the lunar theory. How far they are conclusive, shall be left to future investigation.

But St. Pierre is not content with demolishing the old structure; he has judged proper to erect a new one; and a fair exposition of this system will enable us to determine, by comparison, to which we shall give our suffrage.

It is well known that Sir Isaac Newton and Cassini differed in their opinion respecting the figure of the earth: the former conceiving it to be an oblate spheroid, flattened at the poles; the latter contending it must be oblong, or elongated at the poles.

poles. To aſcertain this point, ſome of the moſt celebrated mathematicians of Europe were appointed to determine, by actual meaſurement, the length of a degree both at the equator and at the pole. They found that the polar degrees exceeded the equatorial, and concluded they muſt conſequently be parts of a larger circle, and, of courſe, that the earth was flattened at the poles. This was univerſally conſidered as deciſive of the queſtion, till the genius of our Frenchman detected a groſs and palpable error in the calculation, which had eſcaped their accurate knowledge and penetration: but, as the elongation of the poles conſtitutes a leading feature in the new theory, I ſhall give it a more detailed examination.

This polar elongation, as he conceives, is ſupported by four direct and poſitive proofs:—the firſt geometrical, upon which he lays the greateſt ſtreſs, and upon which he has ſtaked his reputation; the 2d, atmoſpherical; the 3d, nautical; the 4th, aſtronomical: of all which in order.

The 1ſt, or geometrical proof, is what he calls a demonſtration founded on the meaſurement of the earth, and admitting the polar degrees to exceed the equatorial: here follows the demonſtration: If you place a degree of the meridian at the polar circle on a degree of the ſame meridian at the equator, the firſt degree, which meaſures 57,422 fathoms, will exceed the 2d, which is 56,748 fathoms, by 674; conſequently, if you apply the arc of the meridian contained within the polar circle, being 47°, to an arc of 47° of the ſame meridian at the equator, it would produce a conſiderable protuberance, its degrees being greater.

To render this more apparent, let us always ſuppoſe that the profile of the earth, at the poles, is an arc of a circle containing 47°; is it not evident, if you trace a curve on the inſide of this arc, as the academicians do when they flatten the earth at the poles, that it muſt be ſmaller than the arc within which it is deſcribed, being contained in it? And the more this curve is flattened the ſmaller it becomes. Of conſequence, the 47° of this entire curve will be individually ſmaller than the 47° of the containing arc. But as the degrees of the polar curve exceed thoſe of the arc of a circle, it muſt follow that the whole curve is of greater extent

than the arc of a circle: now to be of greater extent it must be more protuberant: the polar curve, of consequence, forms a lengthened ellipsis. Q. E. D.*

It must be acknowledged that this demonstration is very perspicuous and convincing. How the most celebrated academicians and mathematicians, for nearly half a century, could have overlooked a proposition so plain and simple, can only be ascribed, in the opinion of St. Pierre, to their obstinate and inveterate prejudices. He pursues his victory in a strain of vain and indecent exultation, which would dishonour a more respectable cause; but, perhaps, a little attention will induce us to doubt at least whether the charge of gross ignorance may not, with justice, be retorted on their accuser.

It would have been indeed extraordinary, if men of science had been absurd enough to imagine that a larger arc might be included in a less; but they might suppose, with propriety and justice, that the smaller arc of a larger circle can be included in the larger arc of a smaller circle, which, in the present instance, appears to be the case. In measuring a degree on the meridian, a certain spot is fixed upon, where the elevation of the polar star is taken by a quadrant; from this spot they proceed in a direct line north, till the quadrant indicates an additional elevation of one degree. In proportion as this degree constitutes a part of a larger or smaller

* Let x, Fig. 3. Plate V. be the unknown arc of the meridian, comprehended above the arctic circle A B C; and let D E F be the arc of the same meridian, comprehended between the tropics; these two arcs are each 47°. According to our astronomers, a degree at the polar circle is greater by 674 fathoms than a degree of the same meridian near the equator; the arc x therefore exceeds, in extent, the arc D E F by 674×47, or 31,678 fathoms $= 12\frac{3}{5}$ leagues. The question to be determined is, whether this unknown polar arc x is contained within the circle in the curve A b C, or coincides with it, as A B C, or falls without its circumference, as A x C. The arc x cannot be contained within the circle, as A b C, for it would then be evidently less than the arc A B C; and the more this curve A b C is flattened, the less will be its extent, as it will approach nearer and nearer to the straight line A C; neither can it coincide with the arc A B C, for it exceeds it $12\frac{3}{5}$ leagues. It must belong, therefore, to a curve falling without the circle, as A x C. The globe of the earth, therefore, is lengthened at the poles, since degrees of the meridian are greater there than at the equator.

circle,

circle, a greater or leſs portion of ground will be paſſed over before the deſired elevation is obſerved; and the meaſurement of this ground unequivocally decides whether this degree is part of a larger or ſmaller circle. In this caſe the meaſurement is admitted, but the concluſion denied. St. Pierre ſeems to have ſuppoſed, that the academicians divided the polar arc into 47 parts, and then meaſured one of theſe parts: a thing impracticable and ridiculous. The fact is, that the polar arc, which, if the earth were a perfect ſphere, would contain 47°, does not actually contain ſo many, but perhaps about 46° of a larger circle; and if the polar degrees are parts of a larger circle, as they certainly are, it is demonſtrably evident that the real arc muſt be contained within the ſpherical arc, and, conſequently, that the earth is flattened at the poles *.

[To be continued.]

* Let the circle A B C D, Fig. 4. repreſent the earth as a ſphere, and let P repreſent the polar ſtar, having no ſenſible parallax. Draw the diameter B D, prolonging it to P; draw the tranſverſe diameter C A, the tangent A P, and the line F P, parallel to A P; biſect the quadrant A D equally at F; draw the tangent K L perpendicular to the radius G F, and with the radius B F deſcribe the circle E O M N, and let the ſegment H E F repreſent the earth flattened at the pole; draw the tangent S R to the circle E O M N, perpendicular to the radius B F. An obſerver at A will perceive the polar ſtar P in the horizon; an obſerver at D or E will perceive it in the zenith, or at an elevation of 90°. If the earth be a ſphere, the tangent K L will be the horizon to an obſerver at F, and the angle of elevation P F K is, by conſtruction, 45°; but if the earth is not a ſphere, but flattened towards the pole, as in the ſegment H E F, the tangent S R will be the horizon to an obſerver at F, and of courſe the angle P F S will be the angle of elevation. Now, the angle E G F is by conſtruction 45°, conſequently, the ∠ F G B 135°, and the angles G B F and G F B each 22° 30′. Draw the dotted line E F. Now the triangle B E F is an iſoſceles triangle, and the angle G B F being found = 22° 30′, it follows that the angles B E F and B F E are each 78° 45′, and the ∠ S F B being a right angle, the ∠ S F E is 90° − 78° 45′ = 11° 15′. At E draw the tangent T V, and, for the ſame reaſon, the ∠ V E F = 11° 15′; and conſequently the ∠ E x F = 157° 30′, and the ∠ V x F = 22° 30′. Now the ∠ F Z E is a right angle, therefore the angle P F S = 180° − 90° − 22° 30′, or 67° 30′. The difference, therefore, between the elevation at E and at F will be 90° − 67,30 = 22° 30′, but the difference between the elevation at F and D will be 45°; whence it is evident that a larger meaſurement of ground will be included in a degree in proportion as H E F is the ſegment of a larger or ſmaller circle.

VIII. *Account of a new Operation lately performed with Succeſs in France, for reſtoring Sight in certain Caſes of Blindneſs. By* C. DEMOURS.

C. DEMOURS, who performed this new and ingenious operation, preſented a memoir on the ſubject to the National Inſtitute, which was read on the 15th of June laſt. From this memoir the preſent account has been extracted.

The eye is a ball or globe filled with different tranſparent humours, which are contained in ſeveral membranes. The outermoſt of theſe membranes is called the cornea; it is tranſparent, and placed before the iris, which is the coloured part of the eye, and which itſelf forms a ſecond membrane; but the latter is not tranſparent, and would prevent the rays of light from penetrating to the hind part of the globe of the eye, were it not pierced with a round hole, called the pupil. A little beyond this hole is the cryſtalline humour, forming a kind of lens, through which the rays of light are obliged to paſs; and by means of which they form diſtinct images on the retina at the bottom of the eye. The humour contained in the inſide of the globe is called the vitreous humour; it is perfectly tranſparent, as is alſo the aqueous humour, which is placed between the tranſparent cornea and the cryſtalline.

C. Sauvages, on whom C. Demours performed the operation which is the ſubject of this article, had for ſeveral years an abſceſs in the cornea, in conſequence of which the aqueous humour was entirely waſted, and the tranſparent part of the cornea had become totally white and opake in the right eye, and for about four-fifths in the left eye. This eye is repreſented in the annexed engraving (Plate V. fig. 5). The tranſparent cornea, C, ſuffers to be ſeen towards the upper part but a ſmall portion of the iris. The white ſpot covers entirely the round hole or pupil, which exiſts in the centre of the iris, and through which alone the rays of light can paſs.

C. Demours, taking advantage of the tranſparency which ſtill exiſted towards the upper part of the cornea, made there a ſmall aperture, denoted by the letter A; and having introduced

troduced into that aperture a delicate pair of ſciſſars, made a ſmall hole in the iris, of the ſize of a ſeed of ſorrel, indicated in the engraving by the letter D.

Through this hole, made in a non-tranſparent membrane, the rays of light now enter, and form images on the retina at the bottom of the eye. It is, therefore, an artificial pupil which ſupplies the place of that covered by the white ſpot of the cornea. But as behind this new pupil there is no cryſtalline humour to collect the rays with ſufficient accuracy on the retina, C. Sauvages is obliged, in order to read, to make uſe of a very convex glaſs, ſuch as is uſed by perſons who have undergone an operation for the cataract, and who have loſt the cryſtalline humour. He does not employ the glaſs, however, except in caſes which require more diſtinct viſion; and he ſtill poſſeſſes the invaluable advantage of ſeeing well enough to direct his way, and to diſcern perfectly every object around him. By means of this ingenious operation, ſight in future may be reſtored to the greater part of thoſe who have loſt that faculty by large ſpots or white ſcars, caſes hitherto conſidered as incurable; provided that ſome part of the cornea, oppoſite to the iris, has remained tranſparent.

IX. *On the Cultivation and Uſe of the Syrian Silk-Plant.* *By* J. A. Möller, *Director of the Weſtphalian Patriotic Society* *.

I. *Deſcription.* THE ſilk-plant, *Aſclepias Syriaca* Linn. known by the old botaniſts and our gardeners under the name of *Apocynum Syriacum*, came originally from Syria and Egypt. It is indigenous alſo in North America, and thrives ſo well in Europe, that it would appear that it is ſuited for all countries and climates. The root is perennial, and will laſt from ten to twenty years. In the month of April it throws out, like aſparagus and hops, a great number of ſhoots, the principal ſtem of which riſes to the height of ſeven or eight feet. This ſtem, which is as thick

* From the *Tranſactions of the Academy of the Uſeful Sciences at Erfurt.*

as a man's finger, is ſtraight, round, and ſmooth, and beſet with oval leaves of conſiderable ſize, covered on the upper ſide with dark green, and on the lower ſide with whitiſh down. The plant has a milky juice, which is ſaid to be perfectly harmleſs. The flowers appear about the end of May, and continue till the month of July; there are often from twelve to ſixteen on one ſtem, each of which forms a bunch, containing from thirty to forty ſingle flowers. Each ſingle flower adheres to the bunch by a long thin ſtalk, and has a ſweetiſh odour. Each bunch of flowers is ſucceeded by three, four, and ſometimes ten long, flat, and rough pods, which incloſe ſeveral round, yellowiſh brown, flat and thin ſeeds wrapped up in a beautiful ſhining white kind of ſilk. The ſeeds are winged; a form which nature has given with great variety to many others, in order that they may be conveyed with more eaſe, and to a greater diſtance, by the wind.

II. *Uſes.*—1ſt, The ſilk, which covers the ſeeds in the pods, is the principal part of uſe, and that from which the whole plant takes its name. The pods gradually acquire maturity from Auguſt to the beginning of October, at which time thoſe who cultivate this plant muſt watch with great care for the period of their burſting, in order to collect the ſilk, leſt it ſhould be carried away by the wind, or be ſpoilt by the rain. The pods when collected are ſpread out, to the height of about half a foot, on a net or rack, in an airy place, in order to dry.

The ſilk, which is of a ſhining white colour, from an inch to an inch and a half in length, and exceedingly elaſtic, is then taken out, and being freed from the ſeeds is hung up in thin bags in the ſun, in order that it may become perfectly dry; and at the ſame time it is often ſoftened with the hand, or by being beat. This vegetable ſilk may now be uſed, without any further preparation, inſtead of feathers and horſe-hair, for beds, cuſhions, coverlets to beds, bolſters, and mattreſſes. From eight to nine pounds of it, which occupy the ſpace of from five to ſix cubic feet, will be ſufficient for a coverlet, bed, and two pillows; ſuch beds therefore are exceedingly convenient for travelling. It is not adviſe-

able, however, to use this silk in common for beds instead of feathers, as it is too soft and warm. It requires as little preparation for quilts and counterpanes, and is lighter and warmer than those of common silk.

For spinning, however, notwithstanding its fineness, which approaches near to that of common silk, it is not fit when taken alone, as it is almost too short, and therefore must be used with an addition of flax, wool, or common silk, but particularly of cotton. One-third of this silk with two-thirds of cotton forms a very good mixture for gloves, stockings, and caps. Other mixtures may be used for different kinds of stuffs; but it has been observed that the cloth is much stronger when the vegetable silk is employed for the woof rather than for the warp. Many colours have been applied to such cloth with great success, but as each substance requires a peculiar mode of treatment, more experiments on this subject are necessary: a mixture of one-third vegetable silk, and two-thirds of hare's down, forms hats exceedingly light, and soft to the touch, which have a great resemblance to beaver hats, and are much cheaper.

2d, As soon as the pods have been collected, the stems, which contain a fibrous part capable of being spun, must be cut before they become dry or suffer from the night frosts. They must then be immersed for some days in water, like flax or hemp, and then dried by being spread out on the grass. Care, however, must be taken to ascertain, by experiments, the proper length of time, as too much or too little would be prejudicial. In the last case the flaxy part is brittle, and in the former it loses its strength. After it has been watered it is beaten and heckled: for beating it various kinds of machinery have been invented, and for bruising the stems particular mills are used.

A mixture of the threads spun from the flax of these stems with the vegetable silk and cotton, produces a kind of cloth very proper for furniture. It has been however employed chiefly, with and without an addition of rags, for making all kinds of writing and packing paper; which sometimes is similar to the Chinese paper, and sometimes exceeds in strength the usual paper made from rags.

3d. Both

3d, Both the inner white ſkin, and the external green huſk of the capſules, which contain the ſeeds, might be employed for manufacturing the finer ſorts of this ſilk paper.

4th, That as little as poſſible of this plant ſhould remain uſeleſs, Nature has provided in the ſweet juice of its flowers excellent nouriſhment for bees. According to the author of *Geſchichte Meiner Bienen*, this plant, in the above reſpect, the lime-tree excepted, is ſuperior to all other vegetable productions.

In America a kind of brown ſugar is prepared from the juice of theſe flowers.

The great utility of this plant has been known only within theſe thirty years, though it is probable that it was introduced into Europe about the time of the cruſades. A manufactory of articles from the ſilk of this plant has been eſtabliſhed at Paris ſince 1760, and it has long been employed at Lauſanne, with advantage, for making candle-wicks; but no one has ſhown more zeal in regard to the cultivation and preparation of this article than Mr. Schneider of Liegnitz, who has recommended it in two different pamphlets. In regard to the application of it to paper-making, Mr. Schmid of Lunenburg has made a variety of experiments; and it is much to be wiſhed that others would imitate his example.

III. *Cultivation.*—This plant is propagated two ways, either by the ſeed or by ſlips. In the month of March, after the land has been well dug, the ſeeds are ſown thin, and ſingly, in furrows of the depth of an inch, and covered with earth, which is thrown over them to the depth of half an inch: they are ſecured alſo from the night froſt by moſs or a little light dung. In from four to ſix weeks the young plants begin to appear. The firſt year they produce flowers, but do not bear ſeeds till the ſecond, and do not come to full maturity till the third. In the third year they are tranſplanted. But this method is more laborious, and perhaps ought not to be recommended but in particular caſes, ſuch as when the roots have degenerated, or when they are tranſplanted to a different climate.

The object will be ſooner accompliſhed by ſlips from the roots. As the plant throws out around it long roots with

new

new eyes, theſe muſt be lopped off from the old ſtock either in autumn, when the milky juice in the plant has dried up, or in the ſpring, before it again flows; and are to be cut into pieces of from four to ſix inches in length, but care muſt be taken that they have a ſufficient number of eyes. A freſh inciſion muſt be made in the root before and behind, and they are then to be planted in the ground, to the depth of four or five inches, in an oblique poſition, with the eyes or buds ſtanding upright. Thoſe planted in autumn will produce ſeeds the next ſummer, and thoſe planted in ſpring will bear the ſecond ſummer.

In regard to the further care which this plant requires, the following obſervations deſerve attention:—It thrives in almoſt every kind of ſoil, and even the moſt ſtony, without all further care: but, in order to be brought to perfection, it requires a tender ſandy ſoil; which, however, muſt not be too poor and dry, and which ought to have as much ſun as poſſible. In ſuch ſoil, when properly nurtured, it produces the longeſt, fineſt, and moſt beautiful ſilk.

The ground, before it is planted, muſt be dug up to a good depth, and well dunged. It muſt alſo be well weeded, and kept exceedingly clean. After the crop has been collected, the ſtems muſt be cut cloſe to the ground; and the plants which have died muſt be replaced by young ones. Towards winter they muſt be covered with a little dung, which ought to be ſpread in the ſpring. A ſufficient ſpace alſo muſt be left between the plants. They ought to be planted in rows, and at the diſtance of one foot and a half, or rather two feet, from each other.

Of the ſtems which ſhoot up, only the beſt (perhaps about one half) ſhould be left ſtanding; the reſt, as ſoon as the flowers appear, ſhould be cut, and placed in ſand or earth, to dry up the milky juice that flows from them. Even of the prime plants, it will be proper to ſuffer four or five of the loweſt bunches of flowers to come to maturity. By following theſe cautions, the ſilk obtained will be of a ſuperior quality. The increaſe is very great. In the year 1785, Mr. Schneider began with ſix plants, and in 1793 had a plantation which contained 30,000. The firſt crop produced eight,

the second 356, and the third 600 pounds of silk. If the leaves, after the crop has been collected, be thrown together in heaps to rot, they form an excellent manure for future use. In regard to the preparation of the silk, little is necessary to be said. It may easily be conceived that it will be of advantage to separate that which is long from the shorter part, in order that the former may be employed for spinning. The shorter kind may be used for beds and for hat-making.

This plant may be employed also in the manufactory of paper. Schmid, an ingenious paper-maker near Lunenburg, has made several experiments with the capsules of this plant, which gave the following results:

1st, From the interior white rind of the capsule, mixed with one-third of rags, he obtained writing-paper pretty white, of a good quality, and similar to the silk paper of the Chinese.

2d, From the external green part of the capsules a greenish-coloured paper, which, when sized, was stronger than paper made of rags: it was almost as close in its texture as parchment, and even when unsized did not suffer the ink to penetrate through it. This kind was exceedingly proper for wrapping-paper.

3d, From the stems he obtained a paper so like in every thing to common paper made of rags, that the difference could scarcely be distinguished.

X. *On several new Properties discovered in phosphorised Hydrogen Gas. By* C. Raymond, *Professor of Chemistry in the Central School of Ardeche* *.

We are indebted to C. Gengembre for the interesting discovery of phosphorised hydrogen gas. No chemist before him had observed an elastic fluid which, like the one here spoken of, possesses the singular property of inflaming by the contact of the air alone, without requiring its temperature to be raised, or an ignited body to be presented to it.

* From the *Annales de Chimie*, No. 105.

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The kind of undulating and always increaſing ring to which this gas gives birth; when made to burn bubble by bubble in a place where the air is perfectly calm; the ſplendour and magnificence which accompany its combuſtion when effected in oxygen gas perfectly pure; the ſudden penetration of theſe two gaſes, and their total converſion into water and phoſphoric acid; ſuch were the only facts known which had been intereſting to chemiſts in the hiſtory of phoſphoriſed hydrogen gas, when I endeavoured to diſcover whether this elaſtic fluid did not poſſeſs other properties, which, if they did not exhibit ſo brilliant a ſpectacle as the greater part of thoſe above alluded to, might be worthy of engaging the attention of chemiſts.

The property, long known, which ſulphur communicates to hydrogen, and hydrogen reciprocally to ſulphur, of being able to diſſolve together in water, while both taken ſeparately are perfectly inſoluble, had already given me reaſon to think that the caſe might be the ſame with the combination of phoſphorus and hydrogen; and that theſe two ſubſtances, being previouſly united, might perhaps then become ſuſceptible of partaking in the liquidity of water, and communicate to it new properties nearly analogous to thoſe poſſeſſed by that ſolution of ſulphurated hydrogen gas known under the name of *hepatiſed water*.

To deſtroy or confirm this ſuppoſition, I took a glaſs bottle, and, having filled it with newly diſtilled water, inverted it on the ſhelf of a pneumatic tub, in order that I might introduce into it phoſphoriſed hydrogen gas ariſing from the decompoſition of water by a mixture made with phoſphorus and freſh-ſlaked lime. When the bottle was half filled with phoſphoriſed hydrogen gas I removed it from the tub, taking care to ſhut very cloſely its aperture with my finger, while I ſhook it with force in order to effect more ſpeedily an union of the gas with the water, in the ſame manner as the ſolution of carbonic acid gas, as well as that of ſulphurated hydrogen gas, is facilitated by making uſe of the ſame means.

I ſoon perceived, by the ſtrong adheſion of my finger to the mouth of the bottle, that a conſiderable vacuum had been effected in it, and perhaps, even, that the whole of the phoſ-

phorised hydrogen gas in it had participated in the liquidity of the water.

I then removed my finger, but not without some difficulty, from the mouth of the bottle, that I might examine more accurately the nature and properties of the liquid it contained, and to ascertain whether there did not yet remain some portions of the gas susceptible of inflammation by the contact of the air. But, scarcely was the communication established between the atmosphere and the inside of the bottle, when a loud detonation took place, accompanied with a very brilliant light. I readily judged from this phænomenon, that the whole of the phosphorised hydrogen gas had not been absorbed by the water contained in the bottle; on which account I closed the aperture, in order to prevent the combustion from being longer continued; which would not have failed to produce a considerable quantity of phosphoric acid, and consequently to occasion great uncertainty in regard to the results which I wished to obtain.

The bottle having been closely stopped, I continued to shake it several times, under an idea that I should by these means be able to fix entirely the last portions of the phosphorised hydrogen gas which had remained undissolved.

Hoping that I had succeeded, and impatient to know the new properties which the water saturated with the gas might have acquired, I determined to unstop the bottle a second time in contact with the air. This was soon followed by a second detonation, stronger, indeed, than the former. After this I did not think of stopping the aperture of the bottle, so that a flame exceedingly pale continued for some minutes to issue from it.

When I observed no more apparent signs of combustion, I examined by the smell and taste the liquor remaining in the bottle. Its smell appeared to me exceedingly disagreeable, and altogether different from that of gaseous phosphorised hydrogen; its taste, though very bitter, had in it, however, something insipid and disgusting; and its colour was a little inclining to that of lemon.

When tried with tincture of turnsol it soon made it sensibly red, which I ascribed to the small portion of phosphoric acid which

which muſt have been produced at the moment when the detonations took place, as well as during the diſengagement of the flame which followed the ſecond detonation.

I was then obliged to begin a new operation, that is to ſay, to diſſolve phoſphoriſed hydrogen gas again in water, that I might aſcertain, by employing more caution, in what accurate proportions this ſolution might be effected; and that I might prevent combuſtion alſo from taking place in the inſide of the bottle, which would have become a ſource of error in the concluſion of my experiments.

I had aſcertained with certainty, by means of the quantity of water and gas which I had tried to diſſolve the firſt time, as well as by the detonations which had taken place at the moment when the bottle was unſtopped, that diſtilled water could not diſſolve at the temperature of 47° a volume of phoſphoriſed hydrogen gas equal to its own. I took care, therefore, to introduce into the bottle the ſecond time only about a third of its capacity of the gas. I then repeatedly ſhook the mixture in order to render the union of the gas with the water ſpeedier, and as complete as poſſible; after which I unſtopped the bottle, holding it inverted in a ſmall tub, which I had filled with newly diſtilled water, that I might ſee whether it would become entirely filled with it merely by the effect of the preſſure of the atmoſphere, and that I might thence judge whether the whole gas had been liquefied.

I indeed ſaw a portion of the water in the tub aſcend into the bottle, but I perceived alſo that it was not entirely filled; which confirmed me in the opinion that there ſtill remained a portion of the gas which had not participated in the liquidity of the water. Having then tried to make ſome bubbles of it iſſue out through this liquid, theſe bubbles inflamed ſpontaneouſly by the contact of the air alone; which proved to me that the phoſphoriſed hydrogen gas had not been decompoſed at all by agitation, nor by its contact with diſtilled water; whereas it ſoon loſes its highly combuſtible property when collected in bottles filled with water which has not been diſtilled, or diſtilled water which has been long kept, which I aſcribe to the quantity of air which common water always holds

holds in solution, the oxygen of which, joining a portion of the phosphorus, effects its separation from the hydrogen by converting it into phosphorous oxyd, which being altogether insoluble, deposits itself on the sides of the vessel, without any appearance in this kind of oxydation of any sensible sign of combustion; while the oxygen of the atmospheric air, which participates in the liquidity of the water, being always in that case deprived of a large portion of the light and caloric which are combined with it in its aërial aggregation, cannot produce these two effects in a very sensible manner when it passes in this state of liquid aggregation from one combination to another.

Having again agitated the bottle after closing the mouth of it, I was able, by means of the small quantity of water which had been introduced into it, and of which I kept an exact account, to render the absorption of the last portions of the gas complete; for, having a second time uncorked the bottle in the same distilled water, I then saw it become entirely filled. I think I may affirm, therefore, from these trials, and some others which I made, that water freed from air by distillation can dissolve and liquefy, at the usual temperature, a little less than the fourth of its volume of phosphorised hydrogen gas, and that with this dose it is completely saturated.

This solution thus prepared, and kept free from the contact of the air, always exhibited the following properties:

In colour, it has a pretty near resemblance to sulphur in sticks, though a little less dark; it has a strong disagreeable odour, and an exceedingly bitter, nauseous, and disgusting taste.

When examined in the dark, this solution does not appear luminous; which proves that the phosphorus is intimately combined in it with the hydrogen.

When distilled in a small retort, connected with a pneumatic apparatus, it furnishes, at a temperature a little above that of boiling water, and especially when distilled soon after it has been prepared, a very large quantity of phosphorised hydrogen gas, as pure and as combustible as that obtained by heating caustic alkalies, or quicklime, with phosphorus and a very small quantity of water: what afterwards

wards remains in the retort after the diſengagement of this gas has entirely ceaſed, is nothing elſe than pure water, having neither odour, taſte nor colour, and perfectly reſembling water newly diſtilled.

When brought into contact with atmoſpheric air, this ſolution ſoon ſuffers to be depoſited a remarkable quantity of red oxyd of phoſphorus, and at the ſame time ſuffers to be diſengaged hydrogen gas, which is no longer ſuſceptible of inflammation, except when brought into contact with a body in a ſtate of ignition. If the ſolution be long expoſed to the air, and if the points of contact be frequently renewed by agitation, it becomes completely decompoſed; that is to ſay, is entirely reſolved into phoſphorus, oxyd, and pure hydrogen gas.

Tincture of turnſol and that of violets experience no change in their colour from being in contact with liquid phoſphoriſed hydrogen; which proves that this liquor is neither acid nor alkaline.

The ſulphuric and nitric acids, or the ſimple or oxygenated muriatic, when poured over this liquor, produce no effect worthy of notice.

Potaſh, ſoda and ammonia act in the ſame manner.

The oxyds of mercury and lead are ſpeedily reduced, and immediately converted into metallic phoſphurets, by being mixed with the ſolution of phoſphoriſed hydrogen gas.

When poured into nitrat of ſilver, this ſolution immediately produces a very abundant black precipitate, which does not in the leaſt change its colour, and which, when tried by the blow-pipe, exhibits the characters belonging to metallic phoſphurets.

When brought into contact with a ſolution of mercury by nitric acid, it gives alſo immediately a very conſiderable precipitate, which has firſt a black colour, but which becomes white and cryſtalliſed in proportion as it paſſes from the ſtate of phoſphuret to that of mercurial phoſphat, by imbibing oxygen, either from the nitric acid in which the precipitation takes place, or from the atmoſpheric air with which it is in contact.

A ſolution of lead by the nitric acid is alſo decompoſed by

by the hydro-phoſphorous liquor, but with leſs force than the ſolutions of ſilver and mercury are. There is formed in this decompoſition alſo phoſphuret of lead, which in the courſe of time is converted into a phoſphat.

The ſulphat of copper ſhows alſo, at the end of a certain time, a pretty abundant black precipitate when poured into a ſolution of phoſphoriſed hydrogen. This precipitate, like that obtained by the decompoſition of nitrat of ſilver, retains its colour; which may give us reaſon to believe that it can be converted only with difficulty into a phoſphat.

Sulphat of iron did not appear to me to experience any ſenſible decompoſition till the end of ſeveral days. Nitrat of arſenic, poured into this liquid, did not experience any ſenſible decompoſition till the end of ſeveral days. There was then formed a precipitate of a very beautiful yellow colour under the form of ſmall grains, and which could remain a long time expoſed to the air without experiencing any kind of change. This precipitate was an arſenical phoſphuret.

It reſults from theſe new properties, firſt diſcovered, I think, by myſelf, in phoſphoriſed hydrogen gas, 1ſt, That this gas can unite itſelf to diſtilled water, in the proportion of about a fourth of its volume, when the ſolution is effected at the temperature of 44·5 of Fahrenheit's thermometer *. 2d, That this gas communicates to the water in which it is diſſolved a ſtrong diſagreeable odour, as well as a bitter taſte, which may one day make it be employed with ſucceſs in the treatment of many diſeaſes, either on account of the facility with which this preparation ſuffers itſelf to be decompoſed, or of the part performed by the phoſphorus it contains in the formation of animal matters. 3d, That when water well freed from air has been employed to liquefy this gas, and when care has been taken to keep it thus diſſolved in bottles well corked, it may be preſerved a long time without experiencing decompoſition, ſo that by heating the ſolution you may extract from it, in the ſtate of gas, all the phoſporiſed hydrogen it contains. 4th, That when

* It is probable that at the temperature of freezing water might diſſolve a larger quantity, but the want of phoſphorus prevented me from aſcertaining this fact.

the

the water has thus been freed from all the phoſphoriſed hydrogen it had diſſolved, it becomes pure water; which proves that it was indebted for its new properties to the preſence of this gas alone. 5th, In the laſt place, that this ſolution is capable of ſpeedily reducing ſeveral metallic oxyds, whether alone or diſſolved by acids, and of forming with them, by means of double electrive attraction, water and metallic phoſphurets, combinations which hitherto have been obtained only in the dry way; that is to ſay, by heating metals with phoſphorus, or by decompoſing phoſphoric glaſs or metallic phoſphats by metals and charcoal. Such are the properties which appear to me ſufficiently intereſting to be worthy of being added to the ſtill imperfect hiſtory of phoſphoriſed hydrogen gas.

XI. *On the general Nature of Light. By Mr.* Robert Heron. *Communicated by the Author.*

THROUGH the medium of the Philoſophical Magazine I beg leave to lay before men of ſcience the following opinions, which have lately ſuggeſted themſelves to me as neceſſary and fair inductions from thoſe facts that have come to my knowledge reſpecting light.

1. Light paſſes only through the vacuities in other bodies; does not penetrate their ſolid ſubſtance, ſo as ever to coexiſt with it in the ſame place.

2. It paſſes through thoſe vacuities in ſtraight perpendicular lines, without any loſs of its qualities by attenuation and ſubdiviſion of its particles: or, it is refracted from the perpendicular with a ſubdiviſion of its particles, by which its qualities are altered: or, its paſſage is wholly interrupted, and it is either reflected or abſorbed.

3. Since general attraction acts in bodies in the proportion of their maſſes and aggregation; ſince the more the affinity of aggregation is in any bodies deſtroyed, ſo much the more readily and powerfully do the chemical affinities of compoſition act upon them; ſince the extreme tenuity of light,

the ſubtileſt of all viſible ſubſtances, renders it infinitely ſenſible, equally to the general attractive force and to the chemical affinities of other bodies approaching it:—for theſe reaſons, the ſame *general* and *chemical* affinities, which produce the other changes on material bodies, muſt appear ſufficient to occaſion the different phænomena which light preſents in its various tranſitions.

4. The true primitive colour of light, when unmixed with other ſubſtances, when its particles exiſt together in their natural arrangement and aggregation, when the attractions of other bodies for it act not ſo as to ſcatter or decompoſe its parts, is *white*.

5. Its colours and its peculiar action on the optic nerve depend on the peculiar forms of its elementary particles, on their peculiar aggregation, on their mechanical and chemical qualities, on the modes of its *diffuſion*.

6. It *paſſes without alteration or deflexion through tranſparent bodies* in which the abſolute vacancies are ſufficiently numerous to receive its rays, in which the diſpoſition of theſe vacancies is perpendicular to the direction of the rays, and in which the attractions of the body for the light are not ſo ſtrong as to act on the rays with a diſſolving or a decompoſing force.

7. It is *refracted*, when the vacuities in the bodies on which it falls are not diſpoſed throughout the tranſparent medium perpendicularly to the direction of the rays; and when the attractions, chemical and mechanical, of the parts of that medium are, in reſpect to light, ſo powerful as to occaſion either a tendency to the carrying of the ray into new combination, which ſhall alter its aggregation, or even a partial fixation of it in ſome new compound.

8. It is *reflected* when it falls on ſurfaces which preſent no *rectilinear vacancies*, none but ſuch as are *curves*, through which its rays cannot paſs; and when, at the ſame time, thoſe ſurfaces have no affinities for its particles of ſufficient ſtrength to attract theſe particles into their own compoſition. When theſe circumſtances concur in their utmoſt power the reflexion is complete, and all the falling rays are returned from the ſurface; but the required cir-

cumſtances ſeldom do ſo fully concur. In moſt caſes of the reflexion of light, there is a portion of the falling light either tranſmitted or *abſorbed*.

9. Light is *abſorbed* in the bodies on which it falls, when their attractions fix it in new compounds with their parts; or when they preſent a medium in which it may be for a time ſuſpended, though not carried into actual combination.

10. As the primitive colour of light depends, in a great meaſure, on the conformation and the relative arrangement of its particles, ſo the other colours which it aſſumes in refraction depend likewiſe on changes in the arrangement of thoſe particles which vary the effect of their conformation. While the particles remain in the ſame arrangement the colour is the ſame; and their elementary conformation, and the law of movement to which they are ſubject, continue their illuminating effect to the eye till they are loſt by extreme diffuſion, by abſorption, or perhaps by entire decompoſition.

The permanent colours of the ſurfaces of bodies depend on the ſame laws as the colours of the rays of light. The conformation and the arrangement of the extreme elementary particles at the ſurfaces of all bodies determine, reſpectively, their different colours. *White* ſurfaces, for inſtance, have one conformation and arrangement of the minuteſt particles at their extremities: *blue* ſurfaces have another conformation and arrangement of the ſame particles: *red* ſurfaces another, &c. &c. Theſe extreme particles of the ſurfaces of bodies do not conſtitute light, becauſe they are not unconfined and in motion, have not its peculiar rectilinear elaſticity, poſſeſs not its chemical affinities; but when light is thrown upon them, they become viſible, each ſet of particles under a different colour, juſt as its conformation and arrangement are different. The extreme particles of *blue* ſurfaces have the ſame conformation and arrangement as the particles of light in a *blue* ray; and ſo of other colours.

XII. *Some Account of* FREDERICK AUGUSTUS ESCHEN, *who was ſwallowed up in a Fiſſure of the Snow in the Glacier of Buet*.*

FREDERICK AUGUSTUS ESCHEN was born in 1777, at Eutin, in the circle of Lower Saxony, where his father enjoyed a public office, which gave him an opportunity of having frequent intercourſe with the archbiſhop of Lubec, his ſovereign, by whom he was highly eſteemed. The prince ſtood godfather to this ſon, and inveſted him with the canonicate. Being the eldeſt of a numerous family, young Eſchen was deſtined for the ſtudy of juriſprudence, with a view that he might be fitted for holding ſome public employment; but his father did not confine his education merely to thoſe branches of knowledge neceſſary for that line. He was inſtructed in the ſciences and the fine arts; and, being early familiariſed with both, he applied to them with unabated ardour, and made ſuch a rapid progreſs as attracted the attention of ſome of the moſt celebrated men of Germany, among whom was Voſs, who lived in the ſame village, and was an intimate friend of his father. Young Eſchen was particularly noticed alſo by Count Stolberg, well known by his tranſlation of Plato's Dialogues and his Travels through Swiſſerland and Italy.

At the age of twenty, young Eſchen quitted his father's houſe, and repaired to the univerſity of Jena, where he applied chiefly to philoſophy and juriſprudence; the former of which he ſtudied under Fichte, the diſciple and rival of the celebrated Kant; and the latter under Profeſſor Hufeland, a man as much diſtinguiſhed by his private virtues as by his talents and learning. Natural hiſtory and philoſophy occupied alſo ſome part of his attention; nor did he neglect poetry, to which he had a ſtrong natural propenſity. He tranſlated ſeveral pieces from the Greek and Latin languages, which were almoſt as familiar to him as his own, and to which he had added almoſt all thoſe of Europe. He pub-

* See our laſt Number.

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liſhed alſo various ſpecimens of original poetry, many of which, of the Idyl kind, met with the approbation of Schiller and Goethe, the former of whom inſerted one of them in the Almanac of the Muſes, of which he is the editor. Another of them, entitled *Die Lehre der Beſcheidenheit*, The Leſſon of Modeſty, deſerves to be known on account of the ingenious manner in which the author treats a trifling ſubject little ſuſceptible of poetical ornaments. Eſchen wrote alſo ſeveral eſſays and diſſertations on different points of antient literature, which appeared in ſome of the German journals, and which procured him an acquaintance with ſeveral men of eminence in the republic of letters. It is no little recommendation of Eſchen's character, than we can mention among theſe Schlegel and Humboldt.

In the ſpring of the year 1798, Eſchen undertook a journey to Swiſſerland in order to ſuperintend the education of a young man, chiefly with a view that he might improve his own; and for that purpoſe ſettled at Berne, where one of his countrymen had been before engaged in the ſame occupation. Others ſoon followed him; and ſome young inhabitants of Berne, who had been the companions of his ſtudies at the univerſity, joined themſelves to the circle of his friends. In the enjoyment of their company, with the charms of friendſhip and ſtudy, though amidſt the ſtorms of the revolution, he ſaw two years glide away in peace, and diſtinguiſhed by labours which might have done honour to a longer life. His tranſlation of the Odes of Horace had juſt appeared a little before the fatal event which ſnatched him from letters and from his friends. This production may be reproached with containing too many Latiniſms and bold expreſſions little ſuited to the genius of the German language; but with all theſe faults it will ſtill remain a claſſic work worthy of being ranked among the real treaſures of German literature, and which ought to have inſpired the greateſt hopes of a young man, who, at twenty-three years of age, could venture to undertake a taſk ſo difficult.

The unfortunate cataſtrophe which terminated the life of this intereſting young man has been already detailed in this journal.

journal. We ſhall therefore only obſerve that, about the end of July, this year, Eſchen undertook, in company with M. Theodore Ziemſſen his friend and countryman, a tour to the borders of the lake of Geneva and the valley of Chamouni. They aſcended together the Buet, a high mountain behind the village of Servoz, celebrated by the experiments made there by De Luc and Sauſſure, and which commands a view of the country round Mount-Blanc. They were juſt on the point of reaching the ſummit, and nothing ſeemed to announce that any danger was to be apprehended; Eſchen was walking forwards in high ſpirits before his friend and their guide, when, all of a ſudden, his two companions loſt ſight of him. A thin cruſt of ſnow, which covered a deep fiſſure, had given way under his feet, and he fell into the abyſs; where he periſhed as already related.

XIII. *On the Diſcovery of that Salt known under the Name of Seignette's Salt (Tartrite of Soda). By Profeſſor* BECKMANN.

THIS neutral ſalt, which conſiſts of the mineral alkali (ſoda) and the acid of wine-ſtone (tartareous acid), was prepared and made known by a Frenchman named Peter Seignette, towards the end of the laſt century. The confidence with which the inventor recommended it, and the care he took to conceal the method of making it, had, as is uſual, ſuch an effect, that it was employed in preference to many other medicines, long known, which had been equally ſerviceable; and by theſe means he was enabled, without much trouble, to acquire a fortune. It muſt, however, be allowed that he was a ſkilful chemiſt, who, by his writings and the invention of various other medicines, had obtained conſiderable reputation as a phyſician and naturaliſt. He was eſtabliſhed as an apothecary at Rochelle; publiſhed papers on various natural objects which he had obſerved in his neighbourhood, in the Memoirs of the Academy of Sciences at Paris, as well as in other works, and died on the 11th of March

March 1719 *. He recommended this salt, which enriched him and rendered his name famous, in some small treatises printed, in particular, about the year 1672. He called it sometimes *alkaline salt*, sometimes *sal polychrest*, and sometimes *Rochelle salt*. After his death, his son continued to prepare and to vend it with the greatest success.

Manufacturers and mechanics have been often reproached with the jealousy which they entertain of literary men; but, in my opinion, the latter are the cause of it. It must indeed be confessed, though humiliating for human knowledge, that the most useful discoveries have at first presented themselves to the former, while engaged in the various operations which their employments require; but their merit consists principally in remarking and following phænomena till they produce from them something useful. If they are so fortunate as to succeed, they keep their discoveries secret in order that they may enjoy a monopoly of them; but no sooner has the man of letters heard of a new discovery than he wishes to have a share in the honour of making it known; and his zeal in this respect is proportioned to its importance and the care with which it is concealed; because, in general, he can gain only by rendering it public. The man of letters, however, has a great advantage over the mechanic or manufacturer, as his exertions never fail to be approved; because, by endeavouring to diffuse an important benefit, he appears in the character of a patriot, a friend to mankind, and a citizen of the world; and may thus place the merit of the mechanic or manufacturer in a disadvantageous point of view. This opposition of private interest proves of great utility to the whole society of which both parties are members. When the mechanic or manufacturer makes discoveries, they are communicated to the public by the man of letters; who, by these means, renders them useful; prevents their being hurtful by a monopoly; secures them from dying with the discoverer; and, by investigating the principles on which they

* Some of Seignette's papers are printed in *Bibliotheque Historique de la France par Ferret de Fontelle*, Paris 1778, 5 vols. fol. such as a paper taken from *Memoires de l'Acad.* 1707, p. 115; and also in *Histoire de la Rochelle, par M. Arcère*, Vol. II. p. 424.

depend,

depend, makes their benefit to mankind more certain, and shows how they may be applied in various cases of which the artist or manufacturer never had an idea*. If, by this conduct, he lessen the merit of one, he on the other hand points out employment to many; and gives rise to establishments in which thousands participate, and by which they acquire riches.

Thus Seignette discovered sal polychrest while he was engaged in making soluble tartar (tartrite of potash), and, according to the old opinion, imagining that both the fixed alkalis were the same, used soda instead of the alkali of tartar (potash). By these means he procured, not without surprise, a salt different from the common soluble tartar, which he wished to prepare, and from the other well known salt also. He was induced therefore, to examine it; and having found it to be a new laxative, he recommended it and became rich. The experiments of learned chemists discovered the component parts of this salt; the mode of preparing it was then made publicly known; and, by more accurate examination, the difference, before overlooked, between vegetable and mineral alkali† was determined: by which new light was thrown upon chemistry, and an important service rendered to a variety of arts.

Among those who contributed to bring this salt into repute was Nicholas Lemery, to whom Seignette sent a large quantity of it, which he distributed at Paris, though unacquainted with its component parts‡. Its composition was discovered at the same time, about the year 1731, by two French chemists, Bouldue and Geoffroi. The former published his observations in the Memoirs of the Academy of

* Nam invenire præclare, enuntiare magnifice, interdum etiam barbari solent; disponere apte, figurare varie, nisi eruditis, negatum est. *Plin. Epist.* III. 13.

† Professor Gmelin, in answer to the question, Who first remarked the difference between the vegetable and mineral alkalis, replied that, at any rate, it was first properly defined by Ehl. See G. E. *Stahlii Fundamenta Chymiæ dogmaticæ et experimentalis.* Norimbergæ. 1746. 3 vol. 4to. III. p. 268 and 304.

‡ Lemery Vollkommene Chymist. Dresden und Leipzig, 1734. 2 vol. 8vo. I. p. 512.

Sciences;

Sciences *; and the latter communicated his to Sir Hans Sloane, who caused them to be printed in the Philosophical Transactions †. I shall here observe that chulz ‡ has asserted falsely that Neumann made known the composition of Seignette's salt in his Treatise on Saltpetre; for Neumann's *sal polychrest* is essentially different; and he himself confesses § that he was not acquainted with the Rochelle salt. After the above period, the nature and properties of the mineral alkali were examined with more accuracy by Grosse, Duhamel, Brand (a Swede), and several others ‖.

XIV. *On the Proportions of Charcoal, or Oxyd of Carbon, contained in certain Kinds of Wood and in Pit-Coal; and on a Carburet of Sulphur newly discovered. By* M. Proust ¶.

GREEN oak yields of charcoal 20 *per cent.*; wild ash 17; willow 17; white ash 17; pine 20; heart of oak 19; black ash 25; guaiacum 24 *per cent.*: but all good pit-coals afford 70, 75, or 80 *per cent.* of carbonaceous matter; and there are some kinds which exhibit no signs of containing hydrogen; and which burn without either flame or smoke.

This abundance of carbonaceous matter yielded by pit-coals does not depend on their containing a larger proportion of earth; for good pit-coal yields as small a proportion of ashes as dried pine-wood. The pit-coal of Asturia and Andalusia yields only 2 or 3 *per cent.* of ashes; that of Estremadura not more than 6 or 7 *per cent.*

Besides the known products obtained from the distillation

* *Memoires de l'Academ. des Sciences, Année* 1731, p. 124.

† No. CCCCXXXVI. p. 37.

‡ Chemischen Versuchen, Halle, 1745. 8vo. p. 50.

§ Neumann's Chymie nach Kessels aufgabe. I. 3. p. 160.

‖ An account of the principal writings on Seignette's salt may be found in *Weigel's Chymie*, Griefswald, 1777, 2 vol. 8vo. II. p 225. See also *Georgii Ludov. Enckelmann Diss. de Sale Alkali de Seignette, ejusque Natura et Usu.* Argentorati, 1756. 4to.

¶ From the *Journal de Physique*.

of pit-coals, I have reaſon to believe that they contain a ſmall portion of ſuccinic acid. The collected products from 30 *lb.* of pit-coal afforded me about one dram of a particular ſalt, which, by its ſmell, indicated the preſence of that acid.

But what more particularly arreſted my attention in the analyſes of pit-coal was a very peculiar combination of ſome of the carbonaceous matter with a portion of ſulphur, and where pyrites was not preſent. A coal in the vicinity of Almaden, in Eſtremadura, which contains no pyrites, yields very white aſhes, and from which no ſulphur can be obtained by diſtillation, exhibits, however, as it paſſes from incandeſcence to incineration, proofs of containing ſulphur, which, becoming oxydated along with the coal, makes it ſenſible to the ſmell. A peculiar carburet of ſulphur therefore exiſts in this coal; but combuſtion is neceſſary to its decompoſition. Expoſed to the greateſt heat, this coal neither diſcolours ſilver, nor affects iron in the forge.

This ſingular phænomenon, happily perhaps for the arts, is one of the cauſes which ſo beneficially retard or prolong the combuſtion of pit-coal. Phoſphorus combined with charcoal is much leſs combuſtible than when alone: neither the contact of atmoſpheric air, nor even the combined action of alkalis and water, is ſufficient to ſeparate phoſphorus from its combination with carbon. From the difficulty with which the carbonaceous portion of animal ſubſtances is burnt, the ſame inference may be drawn reſpecting ſulphur; for ſulphur is known to exiſt in a conſiderable proportion in animal ſubſtances. Wool, unqueſtionably, contains much ſulphur, but none paſſes over when that matter is diſtilled. Where then can it remain, if not in union with the carbonaceous reſiduum? That reſiduum ſhould be examined with a view to this point. One thing is certain, that ſoap of wool, prepared in a ſilver veſſel, deeply ſulphurates its interior ſurface.

XV. *Letter from* A. M. Vasalli-Eandi *to* J. Buvina, *Profeſſor of Medicine in the Univerſity of Turin, on Animal Electricity**.

THE electric phænomenon which you obſerved in my electrometer, placed on the back of a diſeaſed animal at the moment of its being attacked by a fit of ſhivering, appears to me to be a neceſſary conſequence of the general theory of electricity, and of the modifications which it experiences in the animal economy. The following is the manner in which, I think, I have proved in my letter on the origin of animal electricity, that man, in the ſtate of health, like all other animals, has parts poſitively electric, while others are negatively ſo.

It appears that, in the animal, the negative part, that of the excretions, is not ſo ſtrong as the poſitive part, that of the blood. But if the change of the animal economy deſtroys the natural boundaries of electricity in the body, on account of the tendency of the latter to bring itſelf into equilibrium, it ought to eſcape and to manifeſt itſelf exactly at the moments when the boundaries are deſtroyed †; that is to ſay, when the virus changes the internal parts, which is proved by the fits of ſhivering: fear and other violent paſſions, as they change the animal economy, ought alſo to produce the ſame effect. Thus you have ſeen the ſtrings of my electrometer, placed on the back of the animal, ſeparate from each other, either during the fits of ſhivering occaſioned by a contagious diſeaſe, or during thoſe produced by fear; you ſee that the ſame theory explains alſo the want of electricity which you obſerved in diſeaſed cats. I am perſuaded that this want will in no caſe exiſt till the diſeaſe has continued ſeveral days, and the animal economy has become deranged. When I concluded my electric experiments on water and ice ‡, I repeated them on ſeveral liquids, animals,

* From the *Journal de Phyſique*, Pluviôſe, an. 8.

† *Journal de Phyſique*, Meſſidor, an. 7.

‡ *Memorie della Società Italiana*, Vol. III.

and vegetables, as well as on different preparations of water. Urine and animal humours exhibited to me the greatest degree of electric difference. You see then that my opinion is supported by facts. As I have found, however, that the blood of those labouring under an intermittent fever is still positively electric *, it might be useful to ascertain in what diseases, and at what degree of those diseases, it loses its electricity. Might not the electrometer be employed to distinguish desperate diseases, and be used, if I may be allowed the expression, as a vitalitometer? But many experiments are still wanting before we can reach that point of perfection in the science of electricity. The discovery of electricity in the torpedo seems astonishing. That of Cotugno, who received an electric shock from a mouse which he was dissecting; and that of Tonso, who had one from a cat; and my electric experiments on rats seem conclusive. But the immensity of nature still presents matter for new researches; and at present, since I have found the electricity of the blood and that of the excretions contrary, I see how much remains to be done to reduce to their just value the opinions of Galvini, Bertholon, Treslan, and Carlieu, on animal electricity. You have pursued the best route, which is to interrogate nature by experiments. Continue to do so, and you will enjoy the satisfaction of having enlarged the boundaries of science.

XVI. *Of Chemical and Mineralogical Nomenclature. By* Richard Kirwan, *LL.D. F.R.S. and P.R.I.A.* †

THE names given to the different substances known in common life, whether occurring in nature or produced by art, are coeval with languages themselves; and whether all were merely conventional, or some of them grounded on some relation to the thing signified, is now of little importance to inquire, as from long habit both are equally immediately referred to the thing signified, without any reflection on the

* *Journal de la Ligne*, Germinal, an. 7.

† From the *Transactions of the Royal Irish Academy for* 1800.

original

original reaſon of their impoſition. Thus, though the name *barometer* was originally impoſed on the inſtrument ſo called, becauſe the terms that compoſe this name, *baros* and *metron*, denote its uſe in meaſuring the weight of the atmoſphere; yet the name is juſt as well underſtood by thoſe who are totally unacquainted with its etymology, as by thoſe to whom this is perfectly known. The inſtrument itſelf, and not its uſe, is denoted by the name, and equally occurs to both; the workman who knows not its uſe, knows what the name immediately ſignifies as well as the philoſopher who employs it, and as well as the German who calls it a *ſchwermeſſer*, whoſe primitive components alſo expreſs its uſe; nor is it better underſtood by either than the name *ſcrew or ſchraube*, which cannot be reſolved into any primitive component terms. Who, on hearing the Latin name of a book, ever thinks of its primitive ſignification—the bark of a tree? Languages muſt have been invented long before either chemiſtry or mineralogy were in any degree cultivated. In both the analytic and ſynthetic branches of chemiſtry as well as mineralogy, many ſubſtances muſt have occurred, to which, in common language, no name was applied, yet the neceſſity of denoting them by ſome name was urgent. Here, then, a difficulty occurred, which ſome ſought to ſurmount by a name *arbitrarily* impoſed, others by names derived from ſome real or fanciful relation of the given ſubſtance to ſome uſe, object, perſon, or particular quality or circumſtance. Thus, in chemiſtry, *alcohol* appears to be a name arbitrarily impoſed on highly rectified ſpirit of wine or very ſubtile powder . *Mercurius vitæ* denotes an antimonial preparation of great efficacy, as *kermes mineral* does one that reſembles that ſubſtance in colour; *Glauber's ſalt*, a particular ſubſtance firſt formed by Glauber; *Epſom*, a ſalt firſt diſcovered in the ſprings near that town, &c. So in mineralogy, *quartz* ſeems to be a name arbitrarily impoſed; and *ſpath*, a name originating from the reſemblance of the integrant *lamellæ* to a blade; and *ſelenite*, from ſome fictitious reſemblance to the moon. The etymology of theſe names was, however, ſoon forgotten or not attended to, and conſequently, ſuch of them

* Some derive it from the Arabic *kahala* exuruit.

as conſiſted of only one word, not evidently denoting ſome falſe relation, might without inconvenience be admitted; but in proceſs of time deſcriptions were admitted inſtead of names, and theſe often falſe or abſurd, as *ſal mirabile*, *ſal ſecretum*, *arcanum duplicatum*, *arcanum corallinum*, &c. The inconvenience and evident impropriety of many of theſe deſignations at laſt attracted the attention of thoſe eminent philoſophic chemiſts, Bergman and Morveau. Morveau, ſo early as the year 1782, publiſhed an excellent memoir on this ſubject in Rozier's journal, and his ſentiments met with the entire approbation of Bergman. The new denominations he ſought to introduce, ſoon however gave way to the more exquiſitely deviſed ſyſtematic appellations grounded on the diſcoveries of Lavoiſier, and the total elimination of the phlogiſtic element formerly admitted. Theſe, in the formation of which Morveau alſo, in concurrence with a few of the moſt eminent Pariſian chemiſts, bore a conſiderable part, were ſince admitted and recogniſed by moſt European chemiſts, and particularly in England. The exceptions that appeared to me reaſonable to the general rules laid down by this highly reſpectable aſſociation, or to ſome of the terms they introduced, I thought of too little conſequence to mention, knowing that the few antient denominations I retained, and the ſtill fewer new ones I introduced, were perfectly intelligible; nor ſhould I attempt at preſent to vindicate them, had I not perceived they attracted the cenſure of many on whoſe eſteem I ſet the higheſt value. Thus circumſtanced, I feel myſelf juſtified in examining the general propriety of thoſe rules and aſſumed principles from which I thought proper to deviate, and of the denominations which I reject.

The firſt principle laid down by M. Morveau is, "that phraſes are not a name; that ſubſtances and chemical products ſhould be denoted by names fit to indicate them on every occaſion, without having recourſe to circumſtances: p. 373. To this principle I give my entire aſſent.

Another rule laid down by M. Morveau is, "that in chooſing denominations we ſhould prefer thoſe which have their roots in the dead languages more generally known, in order that the ſenſe ſhould ſuggeſt the name, and the name

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the ſenſe." It is for this reaſon, combined with the firſt, that I prefer ſingle names, already underſtood, and well known by all thoſe that have attained any knowledge of chemiſtry, to new names derived from the Greek or new unknown barbarous Latin names. Hence I prefer the term *hepar*, denoting in all chemical authors a combination of ſulphur to every baſis except a metallic, to the barbarous unknown Latin term *ſulphuret*; and, with reſpect to metals, I prefer the well known terms *pyrites* and *galena*, to the newly-deviſed *ſulphuret of iron*, or *copper*, or *lead*; the former has the advantage of denoting the particular ſpecific combinations of ſulphur with iron, copper, arſenic, or cobalt, and of excluding not only other ſulphurated metals that do not give fire with ſteel, as the vitreous ſilver ore, ſulphurated antimony, or lead, cinnabar, blende, ſulphurated biſmuth, &c.; but alſo ſuch compounds even of copper and ſulphur as do not give fire with ſteel, as the vitreous copper ore and the gray copper ore, and the purple. On theſe, and many other conſiderations which will preſently be mentioned, I hope the ingenious Mr. Muſhet will recall his wiſh that I had modelled my Nomenclature on the principles of the French ſchool. Phil. Mag. II. p. 156. C. Faujas, though of that ſchool, remarks that theſe uſual names (when otherwiſe faultleſs) cannot, without great inconvenience *, be changed for others either erudite or barbarous. Journal des Mines, XXXV. p. 894. Nor is his opinion in the leaſt invalidated by the note of C. Coque-

* This puts me in mind of ſome Latin puriſts who change known modern names into what they eſteem purer Latin ones. Thus, inſtead of *cancellarius*, they ſay *præfectus juris*; and for which Lipſius Not. ad Lib. I. Politicorum cap. 9. juſtly cenſures the hiſtorian Paulus Æmilius. "Vetuſtatis etiam neſcio quid affectat in nominibus hominum, locorum, urbium immutandis et in veterem formam redigendis, ſæpe erudite, interdum vane, ſed, ut ego judico, ſemper indecore, quorſum *chartierius*, Gallorum cancellarius, *quadrigarius* mihi ſit? et ille ipſe cancellarius appellatur *præfectus juris?* et ubique Rex *Tarraconenſis*, qui nobis et majoribus fuit Arragoniæ? talia infinita ſunt *audacter* et *ambitioſe* innovata et cum fraude pariter ac *cruce* lectoris." The Greek and Roman hiſtorians were frequently guilty of the ſame fault, which occaſions at this day much perplexity.

bert,

bert, who aſſerts that the new are more exact, ſonorous, and ſignificative; for any term is exact and ſignificative when it denotes, without obſcurity or ambiguity, the thing ſignified; and as to *ſound*, it may deſerve attention in poetry, but ſurely, if not exceedingly uncouth, it deſerves none in ſcience. He acknowledges that theſe alterations may cauſe ſome embarraſſment to the preſent race of men, but thinks that ſucceeding generations will bleſs thoſe that introduced them. I think on the contrary that they will curſe them, for obliging them to learn both the new and the old denominations under the penalty of not underſtanding Stahl, Henckel, Margraf, Lemery, Geoffroy, Duhamel, Macquer, Bergman, Scheele and many others of the higheſt merit. Can any one be ſo arrogant as to pretend that theſe immortal authors can become unintelligible without prejudice to the ſcience? Is it poſſible that muſicians ſhould judge more ſhrewdly than thoſe who profeſs to be philoſophers? Yet theſe have rejected all the modern alterations of notation that have been propoſed to them, though attended with ſome advantages, from the ſingle conſideration that, if new modes were adopted, the inimitable compoſitions of the laſt and preſent age would either ſoon become unintelligible, or the arduous taſk of learning both methods of notation would be impoſed on all ſucceeding generations. I am not however for the total excluſion of the term *ſulphuret*; let it be employed to denote the compoſition of ſulphur with any baſis *in general*, whether alkaline, earthy, metallic, oleaginous, ſpirituous or carbonaceous; in this moſt extenſive ſenſe the old nomenclature ſupplied no term, and yet ſome name was wanting; in this ſenſe therefore it may be retained.

Another general maxim advanced by M. Morveau is, that "the denomination of a chemical compound is neither clear nor exact except it expreſſes by names conformable to their nature the ingredients that enter into that compound." This maxim, unhappily too eaſily adopted by the French ſchool, tends to the ſubverſion of the received language of all ſciences, and even of common life. By this rule we are to baniſh the name *water*, and inſtead of it ſubſtitute its component

ingredients

ingredients *hydrogenated oxygen* or *oxygenated hydrogen**; and inſtead of *ice* we are to ſay *decaloricated hydrogenated oxygen*, and for *ſteam*, *caloricated hydrogenated oxygen*. Inſtead of common *ſoap* we are to ſay *oleaginated ſoda*, and for *glaſs*, *ſilicited alkali*, &c.

The French chemiſts it is true retain the name *ſoap*, but in contradiction with their own principles; for they ſurely cannot in this word diſcover the radicals of its compoſition. Nay, Lavoiſier retains the old word nitre and ſalt petre conjointly with that of *nitrat* of *potaſh*, (ſee his Treatiſe of Chemiſtry, vol. I. p. 79 and 232, French) and yet they diſapprove of my retaining a few of the antient denominations that were as well known to all chemiſts as the names *ſoap* or *glaſs* in the language of common life, ſuch as *Glauber*, *Epſom*, and *Sylvian* (rejecting only the term ſalt uſually tacked to them; but evidently ſuperfluous); alſo ſelenite, gypſum, borax and alum—theſe I retain for two reaſons; firſt, becauſe they expreſs their reſpective objects by a *ſingle* name, which appears to me a conſiderable advantage, and ſecondly, becauſe thoſe names continually occur in all treatiſes of chemiſtry publiſhed before the year 1790, and in many ſince, and conſequently muſt be known by all who wiſh to underſtand them.

Morveau and the French ſchool in general reject the names of inventors, "as having no conformity either generic or individual with things:" and for the ſame reaſon they ſhould reject the names *Alexandria* and *Conſtantinople* derived from the founders; and in general, by the ſame rule, all names of places ſhould be changed for ſuch as would expreſs their ſituation. Is it not therefore evident, when the ſignification of names is already fixed and generally known, that they ſhould be retained†, the inconvenience of rejecting them being far ſuperior to any advantages propoſed by the change? Yet, ſtrange to tell, they object to a few new names which I introduced in mineralogy, though exactly founded on their own principles, and not attended with any inconve-

* Fourcroy expreſsly ſays water may be called *oxyd* of *hydrogen*. See St. John's Method of Nomenclature, p. 64.

† 19 Roz. p. 374.

nience, as they denote objects not known by any denomination previouſly aſſigned to them, and even poſſeſſing peculiar properties of great importance. Thus they diſapprove* of the name *muricalcite*, a name which I thought proper to beſtow on ſuch limeſtones as contain a notable proportion of magneſia; and *baryto-calcyte* on ſuch as contain a notable proportion of barytes; theſe ſtones having been diſtinguiſhed by no name, and theſe new names being in conformity with the principles of the French ſchool derived from and grounded on their component ingredients.

Mr. Tennant has ſince ſhown, in an important paper in the Philoſophical Tranſactions for 1799, the evident neceſſity of diſtinguiſhing theſe *muricalcites* from common limeſtones, with which they have always been confounded; and, from the deleterious properties which barytic lime water poſſeſſes, it is evident that the limeſtones that contain any proportion of barytic earth cannot without danger (ſometimes to human life) be confounded with common limeſtones. The ſtone which the Germans without any inconvenience called *apatite*, approaching in our language too nearly the word *appetite*, I have called *phoſphorite* to mark its compoſition; but as the phoſphoric acid has alſo been detected, or at leaſt ſuſpected, in compoſition with argil, to diſtinguiſh this compound from the former I call it *phoſpholite*, a name better ſuited to it than that of *Valentia garnet*, by which it was formerly denoted—a change attended with no inconvenience, as the ſtone itſelf was known in no part of Europe but Spain. To theſe denominations they object their *monotonous terminations*; but theſe terminations are beſt ſuited to our language; they do not recollect that the terminations in *at*, as *nitrats*, *ſulphats*, *muriats*, *carbonats*, *phoſphats*, *oxalats*, &c. &c. are equally monotonous in theirs. They think, or rather magiſterially decide, that minerals ſhould be denoted by the ſame name as ſimilar chemical compounds; a rule that might be admitted if ſuch minerals were not previouſly generally known by other ſingle appellations, and if their compoſition was perfectly ſimilar to analogous chemical compounds. But in the firſt place ſeveral minerals are already

* Ann. Chym. XXXIII. p. 103.

generally

generally known by peculiar proper denominations; for inſtance, *cinnabar*, ſurely a more convenient name than the drawling new name *ſulphurated red oxyd of mercury:* and in the next place they ſhould conſider that chemical compounds being artificial productions may contain only thoſe ingredients which their compound names import. But minerals whoſe principal compoſition is analogous, often contain other ſubſtances alſo, which the new chemical name would not expreſs, and would thus lead to important miſtakes. Thus, for inſtance, *phoſphorite* is commonly contaminated with ſilex, aërated lime, muriated lime and iron, and ſometimes with manganeſe and fluor; whereas the analogous chemical compound on which the name phoſphorated lime is properly impoſed, is free from ſuch contaminations. Subſtances, therefore, ſo different, ſhould certainly be diſtinguiſhed by different names, or at leaſt the word *natural* or *native* muſt be tacked to the name of the chemical compound; and though, with reſpect to foſſile metallic chemical compounds, this inconvenience may often be avoided by the addition of the word *ore*, yet here alſo the antient name often expreſſes ſome other inherent property; thus the term *vitreous* added to ſilver ore denotes not only the compoſition, but alſo the eaſy fuſibility of that particular ore.

[To be continued.]

NEW PUBLICATION.

New Obſervations concerning the Colours of thin tranſparent Bodies, ſhowing thoſe Phænomena to be Inflections of Light, &c. Cadell and Davies, Strand, 1800.

THESE Obſervations are by the author of two treatiſes on the *Origin of the Diverſity of Colours* in the Rays of Light, and on the *many-coloured Coronæ* occaſionally appearing round the Sun and Moon; of which the leading principles have been ſtated in former Numbers of the Philoſophical Magazine.

The object of the preſent treatiſe is to explain, in a manner conſiſtent with the leading principle formerly advanced by the author, another important claſs of the phænomena of light and colours.

Sir Iſaac Newton has repreſented the ſmalleſt parts of all natural bodies as tranſparent. He ſuppoſes thoſe parts to be placed in a medium, inferior in refractive power to themſelves. Thoſe bodies are in his eſtimation tranſparent, which conſiſt uniformly of the ſmalleſt parts and the ſmalleſt intermediate ſpaces: thoſe are black, which have their integrant parts and the intervening ſpaces of a ſize ſomewhat greater: ſuch as are variouſly coloured conſiſt of parts larger than thoſe of either tranſparent or black bodies. The rays of light are, in his judgment, diſpoſed, by a natural indeſtructible quality, to be alternately tranſmitted through oppoſing media, and reflected from them. By this quality, the tranſmiſſions of light, its refractions and its reflections, appeared to him to be in all caſes regulated.

The author of this treatiſe, on the contrary, denies that the minuteſt integrant particles of bodies are known to be tranſparent; that there exiſt within bodies any ſuch refractive media ſurrounding their parts as Newton has imagined; that refraction and reflection depend in any meaſure on the arrangement and conformation of the parts of bodies, which Newton ſuppoſes; or that light is, by a myſterious natural quality, diſpoſed, by turns, to eaſy tranſmiſſion and to eaſy reflection.

He maintains, that all the phænomena which Newton has thus erroneouſly explained, are only ſo many different caſes of the inflection of the particles of light by the attractions of bodies approaching them. According to him, as it ſhould ſeem, light is never, in any circumſtances, made to exhibit a diverſity of colours in its rays, otherwiſe than by the points or edges of approaching bodies exerting on it an attractive force ſufficient to deſtroy the natural conſiſtency of the whole, or at leaſt a part, of the white ray. Refraction and reflection depend upon no peculiar qualities in light, but ſimply on the attraction exerciſed upon it, *according to the general laws*, by the other bodies with which, in various circumſtances, it comes

comes in contact. He has made several ingenious experiments, which are in this treatise related in detail, and of which the common result seems forcibly to suggest the conclusion which he thinks that he has established. Much of the treatise is employed in pointing out the errors as to fact, and the fallacies of induction, which appear in the explanations by Sir Isaac Newton, concerning this part of the science of optics.

To say the truth, we find the facts and reasonings in this essay somewhat less satisfactory than those in its author's two former essays. It is probably the true principle which he has discovered: but he does not, in this instance, see it clearly; he only gropes at it, as it were, in the dark. Perhaps, however, not the author, but the dullness or impatience of the reviewer, may here be chiefly in blame.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ROYAL SOCIETY OF LONDON.

On Thursday, Nov. 6, this learned and useful body held their first sitting since the long vacation, when the commencement of Dr. Herschel's Second Part of his Inquiry respecting Heat and Light was read.

On the 13th the Society occupied itself in reading the conclusion of Dr. Herschel's paper, which contains a detail of a number of most interesting experiments relative to the different quantities of heat and light transmitted through various substances and through different coloured glasses. From Dr. Herschel's experiments it appears that red glass allows most heat and least light to pass through. One experiment is

is detailed, which, though he fays it was courfely conducted, leads him to conclude that the focus of heat falls at the diftance of half an inch from that of light. One hundred and feventy experiments are recorded, from whence he concludes, that, as the laws of the motion of light and heat are eſſentially different, there is the greateſt reaſon to conclude that they are not the ſame fluid.

The Croonian lecture, on the irritability of the nerves, by Everard Home, was begun reading the ſame evening, and concluded at the following meeting on the 20th. In this paper Mr. Home denies the hypotheſis of a peculiar nervous fluid.

SOCIETY OF ANTIQUARIES.

Thurſday, Nov. 6, the Society met for the firſt time ſince the ſummer vacation; on which evening, and on the 13th and 20th, ſeveral very curious fragments of the paintings from the walls of St. Stephen's chapel, repreſenting various ſubjects from the books of Job and Tobit, were exhibited; and a very curious memoir on the ſubject, by their learned Secretary, was read. The Preſident addreſſed the Society, and informed them that the Society had appointed a committee for the purpoſe of ſuperintending the execution of drawings which were making of all the curious remains that have been diſcovered during the alterations that were carrying on at St. Stephen's chapel, and which of late had excited ſuch general curioſity among all claſſes of people.

SOCIETY OF NATURAL HISTORY AT PARIS.

C. Haüy lately read before the Society a note on the cryſtalliſations of iron ore. The intended publication of a treatiſe on mineralogy by this author having induced him to reviſe, with great care, what he had written on cryſtalliſation, he found that he had rectified only in part the incorrectneſs into which he had fallen in regard to the cryſtalline forms of iron ore.

He had already announced, in an extract from his treatiſe, that the cryſtals of volcanic iron were not ſegments of the regular octaedron, as believed, and that the difference between their angles and thoſe of theſe ſegments was more than

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12 degrees. He has since found that they have for their primitive form a rhomboid somewhat acute, in which the angle of the summit is about 87 degrees.

But he thought also, with all other naturalists, that the crystals of the iron of the island of Elba were derived from the cubic form; and he had referred to that form that of the crystals of Framont in dodecaedra composed of two right pyramids incomplete. He had always been struck, however, with a kind of singularity presented here by the cubic form which performed the function of the rhomboid; that is to say, that it was necessary to suppose an axis passing through the two opposite solid angles, which were to be considered as the summits, and the laws of the decrement which took place around the summits were different from those which related to the lateral angles.

He was still more surprised, when, having lately tried to apply theory to a variety of the iron of Framont, which he had not before examined, he observed that it was necessary to suppose it to result from a decrement by twenty rows on the inferior angles of the primitive cube, in order to have results corresponding with observation.

This law, though perfectly admissible, deviated so much from the usual laws, that it inspired the author with suspicions respecting the cubic form itself, and, by the help of goniometry, he measured, for the first time, on the crystals of the island of Elba, the mutual incidence of the primitive faces; whereas he had before confined himself to measuring that of the faces produced by the decrements, either among themselves or on the primitive faces, having no idea that there could be any uncertainty in regard to a form which presented in so sensible a manner the appearance of a cube; and the more so as the facets by which it was modified prevented the difference from being observed. He perceived that this form was a real rhomboid similar to that of volcanic iron; and in that case, this law, which had appeared so singular on the hypothesis of a cube, gave place to a simple law, and every thing, as we may say, was reduced into proper order.

In regard to the varieties of the iron of the island of Elba,

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he found no change to be made in the old laws; becauſe the ſecondary incidences which he had determined in the ſuppoſition of a cube, differed only half a degree from thoſe reſulting from the rhomboidal form. This, ſays the author, is one of thoſe caſes where a quantity very ſenſible of itſelf is diminiſhed by paſſing into certain reſults which depend on it.

It ariſes from theſe reſearches, that all the iron ores which retain a metallic appearance may be reduced to two kinds, very diſtinct from each other; one of which contains ſubſtances that cryſtalliſe in regular octaedra, ſuch as the iron of Corſica; and the other, thoſe having for their primitive form a rhomboid ſomewhat acute, as the iron of the iſland of Elba, that of Framont, and that of volcanoes. The firſt will continue to be diſtinguiſhed by the name of *oxidulated iron*, and the ſecond will be called *oligiſt iron*; that is to ſay, little abundant in iron in the metallic ſtate. It is here ſeen, that a greater quantity of oxygen imprints on the primitive form a character entirely peculiar, by making the regular octaedron paſs to the rhomboid; and this ſeems to indicate two very diſtinct points of equilibrium, which chemiſtry no doubt will determine when it has carried the analyſis of iron ore to a degree of correctneſs, ſuited to that perfection which this ſcience has already attained.

PHILOMATIC SOCIETY.

C. De Sauſſure, the ſon, read lately before this Society a memoir on the influence which the ſoil has on certain conſtituent parts of vegetables. It was formerly believed that the ſoil had no influence on vegetables, but in proportion to the faculty it poſſeſſed of retaining a greater or leſs quantity of moiſture; and it was to this cauſe alone that the difference between the abundance and ſize of vegetables growing in calcareous ſoil, and the ſame qualities in thoſe found in granitic ſoil, was aſcribed. But Sauſſure having remarked that the animals which live in the calcareous lands, that produce theſe vegetables, were larger, fatter, and gave a greater quantity of milk, richer in butyraceous and caſeous parts, than thoſe in granitic diſtricts, conceived an

idea, that there muſt exiſt between theſe vegetables a difference ſtill more important, and which ought to depend more on the nature of the ſoil. Sauſſure therefore has made a ſeries of experiments, with a view to aſcertain the difference which the nature of different ſoils produces in the compoſition of the vegetables which grow in them. That theſe experiments might be concluſive, it was neceſſary to make them very much comparative, that is to ſay, to take the ſame quantity of the ſame vegetables, of the ſame age, growing under ſimilar circumſtances or in the ſame expoſure, at a diſtance from the courſe of ſtreams, and beyond the reach of cattle.

It was neceſſary alſo to repeat the ſame experiments a great number of times in order to obtain mean reſults, and to approach the truth by multiplying probabilities. This C. de Sauſſure did. He began by analyſing the ſtone that compoſed the mountains the plants of which he examined. He then proceeded by the uſual chemical means, which he deſcribes in his memoir, to analyſe the vegetables, with a view to aſcertain the reſpective quantities of natural water, charcoal, earths, and ſalts which they contained. That he might obtain more general reſults he operated on different kinds of plants, viz. the *pinus abies*; *pinus larix*; *rhododendron ferrugineum*; *vaccinium myrtillus*; *juniperus communis*. All thoſe vegetables which belonged to granitic diſtricts contained more water than thoſe of calcareous countries. The ſmalleſt quantity of the extreme difference was as 57 to 58, and the greateſt as 52 to 59. Theſe differences cannot be aſcribed to the quantities of water which might be retained in the mould of the granitic ſoil and that of the calcareous, for they are the inverſe of thoſe preſented by the vegetables of theſe two ſoils. De Sauſſure concludes with Duhamel, that the wood of calcareous countries is preferable, in point of ſolidity, to that of granitic countries.

Proceeding then to a compariſon of the quantities of charcoal contained in vegetables, Sauſſure remarks how difficult it is to eſtimate with preciſion the abſolute quantities of charcoal; the relative proportions of that principle can at moſt be known; and he found that it is more abundant in the

calcareous vegetables; so that it seems to supply the place of that quantity of water which they had less than the granitic vegetables.

Granitic vegetables being more aqueous, ought, according to the observations of Duhamel, to have a softer texture, and consequently to contain more ashes. The incineration of granitic and calcareous vegetables gave differences too difficult to be appretiated; but, however small they might be, they appeared to confirm this observation.

The ashes furnished by incineration having been carefully analysed, Saussure found in those of calcareous vegetables a greater quantity of that earth, and much more silex in the granitic; so that the ashes of the calcareous rhododendron in 100 parts contained 57 parts of the carbonat of lime and 5 parts of silex; while those of the granitic rhododendron contained 30 parts of carbonat of lime and 14 parts of silex. This great difference is one of the most convincing proofs of the influence of the soil on vegetation. As the lime-stone of mount la Salle, where Saussure collected the vegetables the ashes of which he analysed, contains silex, this chemist was desirous of knowing whether vegetables collected on soil entirely free from that earth contained any of it. He therefore analysed the ashes of some plants which had grown on lime-stone totally free from silex, in the mountain of Reculey-de-Thoiry, in Jura, and found in one or two cases only a very small proportion of silex, while in the ashes of the vegetables of Breven he perceived much more calcareous matter than that mountain could furnish. Saussure thence deduces this geologic conclusion, that vegetables cover with calcareous matter mountains which have a base of silex, while the inverse of this does not take place.

In the last place, he made a comparative analysis of the mould in which the plants of Breven and those of Reculey-de-Thoiry grew, in order to determine the relation which ought to exist between that mould, the soil, and the ashes of vegetables which grow in it, and had nearly the following results: The mould of Breven gave 60 of silex, 14 of alumine, 1·16 of lime, &c. That of Reculey-de-Thoiry,

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15 of ſilex, 37 of alumine, 23 of carbonat of lime, &c. It muſt here be remarked, that neither the ſoil nor the plants of that mountain contained any appretiable portion of ſilex.

FRENCH NATIONAL INSTITUTE.

C. Lacepede read a memoir on the ant-eater.

A memoir was read by C. Beauvois on the American fox, and the rabbit of the ſame country. A compariſon of the ſkull of the European fox with that of the American fox, *canis virginianus* Gmel., as well as of the European rabbit with that of America, *lepus americanus* Gmel. has evidently ſhown that theſe two ſpecies have been improperly conſidered by Buffon and ſeveral other naturaliſts as mere varieties of the European ſpecies, and that Erxleben and Gmelin were authoriſed in making them diſtinct ſpecies.

Foxes, like the dog, bear, badger, and ſeveral other animals of the family of the *feræ*, have on the top of the head two ſalient lines, which proceed from the poſterior angle of the orbit and proceed backwards. In the European fox theſe two lines unite at the future of the frontal bone, where they form a ridge more or leſs ſalient according to the age of the individual. In that of America theſe lines are three times as large and as prominent; inſtead of uniting at the frontal bone, they ſeparate from each other, and extend as far as the occipital ridge, where they unite.

The lower jaw in theſe two animals preſents ſtill other very ſenſible differences. Each branch, which in the European fox appears under the form of a well-rounded curve, is ſtraight in the American fox, and forms, with the aſcending branches, an angle of nearly 145°.

Differences equally ſtriking are obſerved between the European and the American rabbits in the elevation and thickneſs of the orbicular apophyſis. The American rabbit never burrows, like that of Europe, and always brings forth two young, like the hare. From all theſe characters it appears that the American rabbit is an intermediate ſpecies between our hare and our rabbit.

C. Decandolle communicated ſome experiments reſpecting the influence of light on certain kinds of vegetables. The

object which the author firſt propoſed in his reſearches was to aſcertain the influence which light has on the ſleep of the leaves and flowers of plants. It appears to the author, that regular viciſſitude of day and night in the ordinary courſe of things renders any reſearches in this reſpect exceedingly difficult; and he thinks that they might be rendered much eaſier if vegetables were expoſed to an artificial light, continued or combined in various ways. For this purpoſe he placed ſix lamps in a dark cellar, and diſpoſed them in ſuch a manner that the illuminated plants had only about 66 or 67 degrees of heat, and were ſheltered from the ſmoke. Theſe ſix lamps gave a light equal to 54 tapers. The experiments which he made with this apparatus are as follow:

Muſtard (the *myagrum ſativum*) and creſſes, ſown, reared, and made to grow, expoſed to this artificial light, had a ſenſibly green colour; but their ſtalks were a little longer than thoſe which grew in the open air.

The leaves of different plants put under water, and expoſed to the light of theſe lamps, produced no oxygen gas for twenty-four hours; afterwards they putrefied, and formed a deleterious gas. This reſult is not aſtoniſhing, for ſix lamps are not equal to the light of the ſun; and it is well known that in the ſhade no oxygen gas is diſengaged.

Branches of the lime-tree, *ſolanum lycoperſicum*, immerſed in water and expoſed comparatively to the light of theſe lamps, an obſcure heat of 69° Fah. and the open air during night, attracted much more water in the light than in obſcurity. Branches of the oak attracted little water in the light, and a great deal when expoſed to heat. Branches of the fir attracted very little in the light. It would appear that this element has a ſtronger action on vegetables that ſhed their leaves than on ever-greens.

The ceſſation of ſuction and of tranſpiration during the night is a real ſleep common to all plants. This name, however, is given to a particular poſition which the leaves and flowers of certain plants aſſume in the night-time.

Linnæus diſtinguiſhes the ſolar flowers into three claſſes; the *meteoric*, *tropical*, and *equinoctial*. C. Decandolle is of opinion that to theſe we ought to add the *ephemeral*, which

flower at a certain hour, and periſh at a certain determined period, which never exceeds twenty-four hours. The marvel of Peru (*mirabilis jalapa*) when expoſed to the light of the lamps for three days, continued to open in the evening, and to cloſe together in the morning, nearly at their accuſtomed hour. The caſe was the ſame in total darkneſs; but, being expoſed to the light of the lamps during the day, they firſt exhibited ſome irregularities, but on the ſecond day they opened in the morning and cloſed up at night. The *convolvulus purpureus*, which unfolds itſelf in the openeſt air at ten in the evening, having been expoſed to the light of the lamps, opened the firſt day at ten in the evening; and next day at ſix.

The rock-roſe, broad-leaved tree-primroſe, bind-weed, ficoides, marygold, and the diamond fig-marygold, exhibited a great number of variations mentioned by C. Decandolle, which it would be too tedious to detail here. We ſhall only add, that the Egyptian fig-marygold (*meſembryanthemum noctiflorum*), when expoſed to light during the night-time, and to obſcurity during the day, opened in the morning and cloſed themſelves at night; and that the *meſembryanthemum ſplendens* and *tenuifolium*, expoſed to a heat of 69°, opened their flowers in a very ſhort time, while the ſame heat had no influence whatever on other plants.

Bonnet, who has endeavoured to explain the ſleep of plants, ſuppoſes that the lower ſurface of the *foliole*, ſuch as thoſe of the falſe acacia for example, is ſuſceptible of extending by moiſture, and that its upper ſurface is ſuſceptible of extending by dryneſs. But C. Decandolle obſerves that the moving cauſe ſeems to act on the inſertion of the *foliole*, and not on the entire ſurface; that this explanation cannot be applied to the leaves the *foliole* of which inclines forwards or backwards; and that it would be neceſſary to admit that the *ſophora* and the *guilandina*, which in the night throw down their leaves, are organiſed in an inverſe manner to the falſe acacia; which is not confirmed by anatomy. The cauſe of the ſleep of plants is therefore really unknown.

In none of the experiments was there any change made in the progreſs of the *oxalis ſtricta* and *incarnata*, but a power-

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ful influence was obſerved on the ſleep of the ſenſitive plant. Several ſenſitive plants, expoſed for three days to the continued light of the lamps, opened and cloſed each day two hours ſooner than the day before; from which it appears that the continuance of light haſtened and did not interrupt their movements. When expoſed to the ſame light during the night, and to obſcurity in the daytime, they exhibited a regular progreſs for nearly two days; after which they expanded in the evening and cloſed in the morning. Their movements were not deranged by total obſcurity, but they ſeem to have been retarded by a heat of 60° to 80°. A heat of 78° rendered the plant ſickly, and deprived it for two days of its ſenſibility when touched.

Theſe facts, according to the author, can be explained only two ways. We may ſay that theſe periodical movements are peculiar to the fibres of plants, and that external circumſtances are only the ſtimulants which excite or retard them; or that the periodical movements continue notwithſtanding the change of external circumſtances, only on account of the habit acquired by the fibres. This laſt explanation appears more probable than the other, becauſe we are already acquainted with ſome facts which ſeem to indicate that plants are ſuſceptible of habit. But, whatever may be the explanation we adopt, we muſt admit as the foundation of it the theory of irritability; that is to ſay, to acknowledge that vegetables are endowed with a peculiar kind of life or force, in conſequence of which their fibres are not affected by external bodies, as unorganiſed beings would be from the mere laws of mechanics.

The Engliſh journals having announced ſome new experiments reſpecting the theory of Galvaniſm, a memoir on which had been preſented to the Royal Society by Profeſſor Volta, theſe experiments have been repeated by C. Robertſon, and by a commiſſion choſen by the Inſtitute.

MISCEL-

MISCELLANEOUS ARTICLES.

ANTIQUITIES.

The lovers of Britiſh antiquities in general, and of Gothic architecture in particular, will be pleaſed to hear that Mr. Lowry, of Titchfield-ſtreet, engraver, and Mr. Alexander, of Newman-ſtreet, well known as draughtſman to the late Chineſe embaſſy, intend to publiſh a ſelection of picturesque and accurate Views (about the ſize and in the manner of Hearn and Byrne's Antiquities) of the croſſes and conduits erected at different times in various parts of this iſland, many of which, from their preſent decayed ſtate, and there being no accurate repreſentations of them, muſt otherwiſe ſoon be irrecoverably loſt. The drawings will be made by Mr. Alexander, and the plates wholly engraved by Mr. Lowry; and, from the known abilities of theſe artiſts, the public have a right to expect a work equal in execution to any thing of the kind hitherto publiſhed.

A proſpectus, with a ſpecimen, will, we underſtand, be ſoon laid before the public.

AGRICULTURE.

The following uſeful hints we copy from a Paris paper:

"A Journal, which has a very extenſive circulation, has lately announced ſecrets for curing the ſmut in wheat, preſerving it from weevils, and for making bread from ſmutty wheat as good and ſalutary as that made from ſound wheat. As a farmer, I take the liberty of giving my opinion reſpecting theſe diſcoveries.

"I can certify with truth, that the method of Trianon, which conſiſts in waſhing the ſeed in a ſolution of lime in water, has always preſerved my wheat from ſmut ſince I have made uſe of it. In my opinion it is impoſſible to find a preventive leſs expenſive. Beſides, I do not employ it but for the fifth part of the wheat which I ſow each year; that which ariſes from it is employed for ſeed the next year, and ſo on in ſucceſſion;

ſucceſſion; but I take great care not to dry the wheat prepared in this manner either in an oven or ſtove, leſt the germ ſhould be injured; it muſt not even be dry when ſown, becauſe the lime would detach itſelf, and injure the ſower.

"In regard to weevils, one of my neighbours has entirely cleared his houſe of them by a very eaſy proceſs. In the month of June, his granaries and barns being entirely empty, he cauſed a number of large ant-hills to be collected in bags, and diſperſed them throughout the places infeſted by theſe inſects. The ants immediately attacked the weevils and devoured them entirely; ſo that not one of them was to be ſeen. Since that period none of theſe deſtructive inſects have appeared on his premiſes.

"The third ſecret is very ſimple: it conſiſts in waſhing the ſmutty wheat in pure water till it no longer becomes black; it muſt then be immerſed in boiling water and dried, ſo that it can be ground. By this proceſs as good bread may be obtained as that made from wheat which is free from ſmut."

CHEMISTRY.

Mr. Klaproth, of Berlin, having lately analyſed *honey-ſtone*, has found that the alumine contained in it is united to an acid, the radical of which is the ſame as that of the vegetable acids, but with different proportions of the carbon and hydrogen.

The vegetable alkali has been found in different minerals. This new fact cannot therefore fail to prove intereſting, and may lead to ſome uſeful facts reſpecting the connection between the vegetable and mineral kingdoms.

THE

PHILOSOPHICAL MAGAZINE.

DECEMBER 1800.

I. *A short View of the new Electric Experiments performed by Dr.* VAN MARUM.

THE experiments which form the subject of this article were performed with the large machine in the Teylerian museum at Haarlem, partly undertaken at the request of other philosophers, and described in his last work on electricity entitled *Tweede Vervolg der Proefneemingen gedaan met Teeyler's El. Mach. Haarlem* 1795, 4to. It must here be observed that, since the last publication respecting these experiments, great improvements have been made in the large machine, and particularly in regard to the cushions. According to the old mode of construction, the cushions were pressed towards the glass plates by means of two screws, in consequence of which it was not possible to maintain an uniform pressure on both sides. At present, however, an uniformity of pressure is obtained by two steel springs, which are applied with hinges to the end of two iron plates, and are kept together by one single screw. As these springs exercise their pressure on the centre of gravity of the cushions, the pressure is uniformly the same in every part. Besides this improvement, the conductor is constructed in such a manner that, merely by turning, it can be employed sometimes for receiving positive and sometimes negative electricity; so that both these kinds of electricity can be communicated to the conductor by changing its position. The collector is no

longer furniſhed with ſpikes, but is perfectly ſmooth and ſomewhat rounded, and has been lengthened from ſix to ten inches.

By the conductor alone experiments were made.

1. Reſpecting the effect of electricity on the pulſe. Dr. Van Marum thought he had reaſon to conclude from his former experiments, that electricity does not increaſe the pulſe. But, as ſome doubts aroſe on this ſubject, which were ſupported by a paper of Meſſrs. Von Troſtwyk and Deiman, he thought it might be of ſome utility, in a ſubject of ſo much importance to medical electricity, to repeat the experiments according to the method of theſe philoſophers. For this purpoſe eleven perſons were ſelected, and the experiment was repeated four times on each, both with poſitive and negative electricity. Theſe perſons were placed in a room which was at ſuch a diſtance from the machine that they could not hear the noiſe it made in turning; they were alſo inſulated, and their pulſe was felt when the machine was in motion as well as when it was at reſt (which laſt circumſtance was unknown to them), and the beats were told by a good obſerver by means of an excellent watch. In ſome ſingle caſes a few beats more were obſerved, but on the whole there was no particular increaſe of any conſequence. In general, however, there was great irregularity in the pulſe both during the time the perſons were electrified, and during the time the machine was at reſt.

2. Reſpecting the increaſe of inſenſible perſpiration during the time the perſons were electrified. Dr. Van Marum employed for this purpoſe a very ſenſible balance, one ſcale of which was inſulated by means of a ſilk cord. On this ſcale he placed a boy, eight years of age, connected with the conductor, and brought the balance into equilibrium. He then examined the loſs of weight ſuſtained in half an hour before the boy was electrified, and found it amount to 280 grains; after which the machine was turned for half an hour, and the loſs at the end of that time amounted to 295 grains. By a ſimilar experiment on another occaſion, the loſs of weight before being electrified was 330, and after being expoſed to electricity only 310. A girl of ſeven years loſt, before being

electrified, 180; and when electrified, 165 grains. A boy of eight years and a half loſt unelectrified 430, and when electrified 290. Another of nine years, unelectrified 170, electrified 240. As the laſt boy was exceedingly quiet during the experiment, it was thought that the increaſe was the conſequence of electricity; on this account he was ſeveral times ſubjected to the experiment, and the reſults were in the unelectrified ſtate 550; in the electric 390, 330 and 270, 550 and 420. In moſt of the experiments it appeared that there was rather a decreaſe.

3. Reſpecting the irritability of the veſſels of vegetables as the cauſe of the aſcent and deſcent of ſap.—The reſult of theſe experiments (*viz.* that from the cut ſtems of different kinds of euphorbia, and other plants of the like nature, when expoſed to ſtrong ſparks, no more ſap flows, as, by the irritability of the fibres being deſtroyed, the veſſels are rendered incapable of contracting themſelves) is already well known.

4. Reſpecting the exiſtence of caloric in the electric matter. —Dr. Van Marum cauſed a conductor of very thin braſs plate, five inches in diameter and eleven inches in length, to be conſtructed with a cavity in the middle, in which he placed the bulb of a very ſenſible thermometer, and ſuſpended it by ſilk ſtrings near the conductor of the large machine. Neither by poſitive nor negative electricity, however, did their appear the leaſt ſign of the thermometer riſing. As charcoal is an excellent conductor, he introduced the bulb of the thermometer into a cavity made in a piece of that ſubſtance; but ſtill there was no ſign of heat. From this it appears to him, that the ſigns of heat exhibited by electricity may ariſe only from the great velocity with which the electric matter paſſes through bodies, and that the fuſion or combuſtion of theſe bodies thence reſulting may be occaſioned by the friction thus produced. If a ſtream of electric matter be conveyed to the bulb of a thermometer, it immediately riſes as Van Marum found, and often from 80 of Fahrenheit to 100 and more: but this experiment cannot be conſidered as a proof of the exiſtence of caloric in the electric matter, as Cavendiſh found the electric current decompoſes the atmoſpheric air, by which means ſome caloric may be diſ-

engaged from it. To afcertain the truth of this conjecture, Van Marum introduced a thermometer into a receiver between two conductors, and, having exhauſted part of the air, threw the electric ſtream on the bulb. The thermometer, however, roſe higher than in the open air, or to 120 degrees. The air in the receiver had been rarefied to $\frac{1}{72}$. That he might proceed with ſtill greater certainty, he performed the experiment in oxygen gas and azotic gas rarefied in the ſame degree; but in both caſes the thermometer roſe as much as before. Van Marum thought he ſhould find another proof of the above opinion, by trying whether the electric matter was able to convert aqueous liquids into expanſible fluids; for, as the elaſticity of all ſuch fluids is aſcribed to their mixture with caloric, it ſeemed reaſonable to conclude that caloric muſt exiſt wherever it is poſſible to form expanſible fluids. Prieſtley converted vitriolic ether, by means of electric ſparks, into inflammable gas, and obtained the like reſult from oil of turpentine, ſpirit of wine, and ammonia; but Dr. Van Marum, from theſe ſubſtances, obtained only very little gas, though his machine worked with much greater ſtrength, and the ſmall quantity which was obtained was again ſoon abſorbed; and therefore he is of opinion that theſe gaſes were rather expelled from the above ſubſtances by the electricity, than prepared by it from their component parts. With ether and ammonia the quantity was a little larger; but, as theſe ſubſtances are exceedingly volatile, it could not be determined with certainty that the air was produced from the caloric of the electric matter; becauſe ſeveral liquids acquire their liquid form merely from the preſſure of the atmoſphere. Dr. Van Marum conceived the idea of making the experiments in vacuo with other fluids, becauſe in that caſe a ſmall quantity of caloric might produce air. For this purpoſe he employed the Torricellian vacuum, and, having put platina wires into ſeveral barometric tubes $\frac{1}{2}$ inch in diameter, fuſed the glaſs around them at a lamp; he then inverted the tubes, and filled them with quickſilver in ſuch a manner that $\frac{1}{3}$ inch of each remained empty. Into this empty ſpace he introduced the fluids, through which the electric ſparks were to be conveyed; then cloſed the aperture, and again inverted

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the tubes, that the fluid might aſcend to the upper part of them. The exhauſted ſpace left by the deſcent of the mercury was ſome inches in length, which appeared to him to be the moſt advantageous. He then held the tubes in a vertical poſition in a veſſel filled with mercury, and, placing a ball three inches in diameter on the wires at the top of the tubes, cauſed the ſparks from the conductor to fall upon it. The quickſilver in the tube was at the ſame time connected with an inſulated ball by means of a wire.

The firſt experiments were made with water, carefully purified by boiling and by the air-pump. When the ſparks ſtruck the water through the vacuum, a conſiderable quantity of air ſeemed immediately to be produced, ſo that in three minutes the mercury fell 1½ inch. During the next five minutes the mercury fell only ¼ of an inch, and the production of air then totally ceaſed. After three days, the air which had been produced did not appear to be in the leaſt leſſened. By another experiment of the like kind ſuch a quantity of air was produced that in four minutes the mercury fell 3 inches 4 lines; but next day the air was leſſened 1 inch 8 lines: the remainder retained its elaſticity.

In an experiment with alcohol, ſuch an abundant quantity of air was produced that at firſt the mercury fell ½ inch at each ſpark. The production of air, however, decreaſed in proportion to the falling of the mercury. Two experiments of the ſame kind gave more ſtriking reſults, which in other reſpects were ſimilar to the former.

In an experiment with cauſtic ammonia, a column of air of 21 inches was produced in five minutes. Carbonat of ammonia gave a column of 18 inches, and camphor one of 6½ inches in the ſame time. The air produced by alcohol, when tried by a teſt, was found to be pure inflammable air: that from camphor was found to be nearly as free from mixture: that, however, from both kinds of ammonia conſiſted of inflammable air mixed with azot. From this it appears that the electricity had ſeparated from each other the two component parts of the ammonia, hydrogen and azot. Dr. Van Marum imagined that the air produced from the water would conſiſt alſo of oxygen and hydrogen, and on that ac-

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count tried to inflame it, but without fuccefs: he condenfed the air he obtained till it almoft equalled in that refpect atmofpheric air, by immerfing the barometric tube in a wider one filled with quickfilver; but inflammation did not take place until a little oxygen gas or atmofpheric air was introduced into it. Hence there was reafon to conclude that inflammable air only was obtained from the water; and Dr. Van Marum acknowledges that it is difficult to explain what came of the oxygen, the other component part of the water. Why, fays he, did it not form itfelf into oxygen gas with the caloric produced from the electric matter? But it is poffible, adds he, that this laft formation may be much more difficult than the former; and, as it appeared from former experiments, that the electric fpark decompofed oxygen gas, its oxygen may have paffed into the mercury, and the calorie may have efcaped: no figns of oxydation, however, were obferved on the mercury.

All the kinds of air produced in this manner, that from water excepted, fully retained their elafticity; for even at the end of a year no leffening of them was obferved in the tubes, though kept under a preffure equal to that of the atmofphere. Electricity, therefore, had effected in thefe experiments what had been formerly afcribed to caloric, and they feemed to prove that caloric exifts in the electric fluid. This much however is evident, that the electric fluid is not caloric itfelf, otherwife it muft heat thofe bodies through which it paffes; but this is not the cafe: it appears alfo that in the electric fparks it is combined with fome other fubftance which prevents it from communicating heat to bodies; and perhaps it is in a condition to do fo only when it is difengaged, and becomes free by the electric matter being decompofed.

Whether this other fubftance may not be the matter of light, and whether during thefe experiments it paffed through the fides of the glafs, as no figns of any other matter on the glafs could be obferved, he could not decide. All the caloric difengaged was not, however, employed in the formation of inflammable air: a part of it remained free, and heated the tubes, which within five minutes were at a temperature equal to 150 degrees of Fahrenheit.

5. Ex-

3. Experiments made to determine whether it was poſſible by electric ſparks to decompoſe certain ſubſtances, or to change them in a ſenſible manner.—As Dr. Van Marum, in the years 1785 and 1787, was able to decompoſe nitrous and alkaline air by electric ſparks, he wiſhed to try the ſame experiment with other ſubſtances. For this purpoſe he employed tubes of from 13 to 14 inches in length, and from 3 to 4 lines in diameter, into the ends of which he introduced platina wires, and fuſed the glaſs around them. When the bodies to be examined required mercury, he filled the tubes with that ſubſtance, and introduced the bodies in ſuch a manner that they floated about an inch over it. Above the bodies an inch of air was left, that the ſparks might be conveyed to them with greater force; for he had been taught, by experience, that the ſhocks are of the utmoſt importance in experiments of this kind. However, he could not venture here to employ any atmoſpheric air; as this air, when decompoſed, gives nitrous acid, which would have mixed with the products obtained. The fitteſt for this purpoſe were vital air and azot. When he tried ſuch ſubſtances as attack mercury, the whole tubes were filled with the acid, and a platina wire was immerſed in it, ſo that its upper end was an inch deep below the ſurface of the acid. This wire ſerved inſtead of the mercury as a conductor.

Having introduced concentrated ſulphuric acid into this laſt apparatus, and conveyed to it, for a quarter of an hour, poſitive or negative ſparks, no ſigns of any change were obſerved. The caſe was the ſame when it was ſtrongly heated or rarefied.

Fuming nitrous acid gave, in the courſe of five minutes, a column of two inches of an aëriform fluid; but in a quarter of an hour very little of it remained. It appears that the caloric of the electric matter had given the acid a gaſeous form, but in itſelf no change could be perceived.

Common nitrous acid gave a column of air of half an inch, which diſappeared alſo when the electric matter ceaſed to act.

Common fuming muriatic acid exhibited the ſame phænomena as the former. The hyperoxygenated did not pro-

duce

duce the leaſt gas; it appeared that the caloric had no great tendency to unite itſelf with the oxygen.

Carbonat of potaſh, or fuſed ſalt of tartar, treated for a quarter of an hour over mercury with ſparks, experienced no change.

Concrete volatile alkali gave, between mercury and air, ſo much gas that the whole tube was filled with it. In this caſe the product was partly inflammable air and partly azot; and it appears from theſe experiments that the formation of gas from both theſe component parts takes place as well in air as in vacuo.

Tincture of lackmus did not become red, though expoſed to ſparks for half an hour.

Profeſſor Volta had requeſted Dr. Van Marum to convey ſparks to fuſed ſaltpetre, in order to try whether a decrepitation would take place. This, however, was not the caſe; and, after cooling, the ſaltpetre did not appear to be in the leaſt alkaliſed.

As oxygen ſeparates itſelf from horn-ſilver in the light of the ſun, Prieſtley firſt propoſed to Van Marum to expoſe it to electricity; but no air could be obtained from it, either between the mercury and water, or in the Torricellian vacuum.

Solutions of ſilver, copper, iron, lead, and quickſilver, in nitrous acid, and of gold and tin in aqua-regia, did not give the leaſt precipitate in a tube furniſhed with a platina wire. With ſilver, lead, tin, and quickſilver, a little aëriform matter was obſerved, which however did not amount to above one-fourth of an inch, and was again immediately abſorbed after the experiment.

6. Experiments which ſhow that charcoal contains hydrogen.

Theſe experiments were made in conſequence of a viſit from Landriani, on the 10th of November 1784. Lavoiſier's combuſtion of charcoal in oxygen gas had proved merely that carbonic acid is produced from charcoal and oxygen gas; but he as little proved as any of the antiphlogiſtians, by a direct experiment, that the charcoal or the carbonic acid, obtained by its combuſtion in vital air, carried with it no

water.

water. The fixed air in this experiment was obtained by heat from a mixture of charcoal-duſt well dried and freed from air, and red precipitate expoſed to heat. In order that the veſſels might be freed from all moiſture they were expoſed to a ſtrong heat, and the quickſilver employed in the apparatus was boiled. In order to try whether the carbonic acid obtained contained any water, ſtrong ſparks were made to paſs through it; and the operator watched with great care to obſerve whether water would be produced, and whether a long ſpiral iron wire, employed in the apparatus, would be oxydated. The fixed air itſelf occupied in the tube the length of nearly 4 inches $6\frac{1}{2}$ lines before being ſubjected to electricity, and the diameter of the tube was 7 lines. As ſoon as the ſparks entered it, the operators ſaw with aſtoniſhment that the column of air gradually increaſed, and, after being electrified 16 minutes, the air in the tube occupied a ſpace of 5 inches 1 line, which gave an increaſe of almoſt $\frac{1}{10}$ of the whole. They then waſhed the fixed air in cauſtic alkali till its volume ceaſed to be leſſened, and the reſiduum in the tube amounted to two inches. The flame of a taper being then applied to the aperture, this electrified reſiduum inflamed, and therefore ſhowed that it conſiſted entirely of unmixed inflammable air. As this reſult did not coincide with what we are taught by theory, it was reſolved to repeat the experiments, and to employ double care in order to drive off the moiſture. But as greater attention was now paid to what took place during the revival of the mercury, it was obſerved that ſome vapour was depoſited in the upper part of the flaſk employed for the experiment, as well as in the tube through which the produced air was made to paſs. It was ſuppoſed on the firſt view that this vapour was ſublimated mercury; but it ſoon formed itſelf into ſmall drops of water, which increaſed in ſize, ſo that no doubt now remained reſpecting the production of water. The reduction was then ſuſpended, and the whole apparatus was heated and dried as well as poſſible; but, on continuing the experiment, the drops again appeared. Now, as it was impoſſible that this water could ariſe from the moiſture of the veſſels, it ſeems proved to Van Marum that carbon not only

contains the baſe of carbonic acid, but that alſo of hydrogen gas. But even if this experiment had proved the exiſtence of hydrogen in charcoal, it would not follow that the inflammable air thence reſulting effects the reduction of metallic oxyds in the way underſtood by the Stahlians; it would ſhow nothing more than that charcoal is not a ſimple ſubſtance *; for, if it were ſo, it would not produce water, but would ſimply join the the oxyd and reduce it.

[To be continued.]

II. *Of Chemical and Mineralogical Nomenclature.* By Richard Kirwan, *LL.D. F.R.S. and P.R.I.A.*

[Concluded from Page 179.]

Another inconvenience ariſing from denominations aiming to expreſs the compoſition of the objects on which they are beſtowed, is, that they often cannot expreſs the proportion of the compounding ingredients without conſiderable embarraſſment; this proportion nevertheleſs is a circumſtance often of great importance, as it induces an important difference in their properties, and the embarraſſment is greater when the compounding ingredients are numerous. To obviate the firſt, in one particular inſtance, the French ſchool have very properly aſſigned different names to compounds holding different proportions of oxygen and poſſeſſing different properties in virtue of that difference of proportion, as *acids* and *oxides*; but ſeveral of the vegetable acids differ from each other only in the proportion of ingredients, which cannot, even if fully known, be expreſſed in detail on every occaſion. So, to obtain a diſtinct knowledge of the various combinations of ſulphur, or ſulphur and hydrogen, with different baſes, is of the utmoſt conſequence to any one that wiſhes to obtain any inſight into the phænomena preſented by mineral waters during their analyſis, or of the nature of other ſulphurated compounds: on this obſcure and intricate inveſtigation moſt

* It is well known that charcoal retains water with great obſtinacy and it is extremely probable that, in ſpite of the care uſed in this experiment, ſome of that liquid had been left in the charcoal employed.

certainly the eminent abilities of Fourcroy, and the consummate skill and sagacity of Berthollet and his associate Welter, have thrown the clearest light; yet I must own that the terms employed by the two last to denote the different compounds appear to me very perplexing, though the best that could be chosen on the principles of the new nomenclature *. I flatter myself, therefore, that a short explanation of them, and of the names I would wish to substitute in their room, will not be unacceptable nor out of place on this occasion.

Sulphure, Sulphuret of the English.

By this the French understand the compound of *sulphur singly* with any bases except hydrogen: these compounds I call *hepars* if unmetallic, but if metallic I apply their antient names.

Hydrogene Sulphuré, Sulphurated Hydrogen.

This complex denomination expresses the union of sulphur with hydrogen. I call it by its antient name, *hepatic air.*

Souffre Hydrogené.

This term expresses the union of sulphur with hepatic air. The sulphur abounds, and it does not form a permanent air, but assumes an oily form. English neologists would, I presume, have called it *hydrogenated sulphur:* it is plain that this name does not express all the ingredients, for it is to hepatic air, and not merely to hydrogen, that the sulphur is united; I call it therefore *hepaticated sulphur.*

Hydro Sulphure.

Hepatic air is capable of uniting to various bases, and even precipitates sulphur from them if previously united to them; on these compounds Berthollet bestows the above denomination. The English, I suppose, would call them *hydrosulphurets*; I call them *hepatules.*

Sulphure Hydrogené.

This denotes the union of hepaticated sulphur *(souffre hydrogené)* with any basis: the English, I suppose, would call

* See Ann. Chem. XXV. p. 230. and New Rex. Jour. III. p. 436.

theſe compounds *hydrogenated ſulphurets:* this appellation does not expreſs the compoſition. I employ the term *hepaticated* to expreſs this compoſition. Berthollet has diſcovered that fixed alkaline hepars, when diſſolved, or even moiſt, are always in this ſtate, and conſequently, that an alkaline hepar cannot exiſt but in a dry ſtate.

In the caſe before us, we ſee the confuſion and perplexity occaſioned by a ſtrict adherence to the rule recommended by Lavoiſier *, that a conformity or connection ſhould be maintained between the names of a baſis and its compounds: in ſome caſes it is highly proper, and hence the different degrees of oxygenation of certain acids are very properly indicated by a ſlight alteration of the termination of the name of each, as *ſulphureous* and *ſulphuric*; but theſe names indicate only *extreme ſtates*, the firſt only the ſmalleſt degree of oxygenation conſtituting an acid, and the laſt the ſtate of *perfect ſaturation:* now, this ſame acid commonly occurs in neither of theſe ſtates, but in a ſtate participating more or leſs of both; theſe names, therefore, applied to it when thus circumſtanced are falſe; and as in this intermediate ſtate it has always been known by the name of *vitriolic acid*, I think this name ſhould ſtill be retained. Nay, the ſulphureous acid is itſelf capable of two very different ſtates, as may be ſeen in the 6th volume of De Machy's edition of Junker, p. 143. So the term *nitric* is very proper to denote the full ſaturation of the nitrous baſis with oxygen. But the term *nitrous*, employed to denote the ſmalleſt degree of oxygenation neceſſary to convert this baſis into an acid, is improper, as it has evermore conveyed a different idea. Hence I expreſs this loweſt extreme of oxygenation by the term *mephito nitrous*, as the radical primary baſis may be called *mephite*, or mephitic air, inſtead of the new coined name *azot*. And the term *nitrous acid* may ſtill denote, as it has evermore done, the mean ſtate of oxygenation; that indeed in which it is uſually found, and for which the French ſchool have no name but that of one or other of the extreme ſtates, which muſt therefore be falſely applied. The term *epinitrous air*, introduced by the learned and ingenious Dickſon, may be uſed to denote what Doctor

* Lavoiſier, p. 72 and 73.

Prieſtley

Prieſtley called *dephlogiſticated nitrous air*, at leaſt until its nature is better developed.

It is in vain that the authority of Bergman is invoked to countenance the ſuppreſſion of the antient names of Glauber, Epſom, &c. rejecting only the oſtentatious additions made to ſome of them, as *ſal admirabile Glauberi*, &c. 4 Bergm. p. 257. It is true he affirms that *the beſt names are ſuch as indicate the compoſition, or ſome eſſential property*; but of this ſort he gives no example, nor rejects any old names merely on this account: on the contrary, where *expreſſive names* cannot eaſily be had, he tells us it is better to apply ſome that convey no determinate expreſſion, p. 259; and this is often the caſe where different proportions or numerous ingredients are to be denoted.

Hence I am far from rejecting, but on the contrary applaud the ingenuity of the inventors of the terminations of *at* and *ite* to denote the different proportions of oxygen in the acids contained in different compounds, as *ſulphats*, *ſulphites*, *nitrats* and *nitrites*, &c. as ſuch general names were undoubtedly wanting, and the old ſchool afforded none. But the welcome admiſſion of theſe does not require nor imply the diſmiſſal of ſuch of the old as were faultleſs, and enfranchiſed by preſcription.

A highly valued friend ſuggeſted to me, that the uſe of the old names was a departure from the ſyſtem on which the new denominations were founded. I replied, that ſyſtems were the creatures of convenience, and ſhould be adhered to only as far as they promoted it; nitre, epſom, borax, &c. are much ſhorter (and equally well known) than nitrated potaſh or ſulphat of magneſia; and Lavoiſier himſelf preſerves the name borax; he might as well have preſerved that of epſom.

The fifth rule (very properly) laid down by Morveau with reſpect to names is, that they ſhould be adapted to the genius of the language; conſequently, if old names be retained, they ſhould be employed in the true uſual ſenſe of their own ſignification in that language, and neither extended beyond it nor reſtricted within narrower bounds: on this ground I reject the term *potaſh*, employed to denote the vegetable fixed alkali

alkali in its pureſt ſtate; for that name, both in Engliſh and French, has always denoted an impure alkali; but the pureſt alkali of this ſort, having been formerly denoted by the name of *ſalt of tartar*, a name certainly improper, I ſubſtitute in its room the unexceptionable name *tartarin*, which, by its affinity to the former, eaſily ſuggeſts its ſignification, and is moreover attended with a ſmooth flowing adjective, *tartarinated*, which is often wanting. For the ſame reaſon I reject the name *ammonia* to expreſs the *volatile alkali*, as the name *ammoniac* has always been employed to expreſs the combination of a volatile alkali with an acid, and, if no particular acid was expreſſed, the muriatic was underſtood: inſtead of volatile alkali, which is a compound denomination, I ſubſtitute *volalkali*, whoſe ſignification cannot be miſtaken. Its adjective is not, indeed, quite ſo happy: inſtead, then, of *volalkaliſed*, I uſe the word *fuliginated*, which eaſily indicates the ſame idea. Theſe are the only new chemical names I employ.

The term *oxide* is alſo unſuited to our language, in which it naturally expreſſes the *hide of an ox*. In pronunciation they cannot be diſtinguiſhed; in its ſtead I would uſe *oxat* or *oxidat*, and inſtead of *oxided* I would ſubſtitute *oxidated*. The application of either of theſe terms to metallic ſubſtances in an oxidated ſtate is generally ſuperfluous, as ſuch ſubſtances are already denoted, and known under the name of *metallic calces*. Guyton * has lately proved that diamonds are the pureſt carbon; yet ſurely even the French ſchool will not attempt to ſuppreſs that well-known name, and exchange it for carbon. Neither, I ſuppoſe, will they call charcoal an oxide of carbon, though proved to contain ſome portion of oxygen; and for the ſame reaſon I ſhall not exchange the well-known term *plumbago* for that of *carburet of iron*, though with reſpect to ſimilar compounds of other metals the term *carburet* ſhould be employed.

This ſyſtem of conciliation the French ſchool rejects with

* It is with much regret and reluctance I mention this gentleman under this new name, as he was generally known, and gained immortal fame, under that of Morveau; hence I ſhall ſtill uſe this in quoting his former works.

diſdain.

diſdain. Guyton tells us, "it is ſo much more difficult to conceive, as it is an evident ſacrifice of principles to habits*;" as if the ground of their ſyſtem of nomenclature were univerſally allowed, and afforded rules ſo ſtrict and general as to enjoy the ſingular privilege of admitting no exception! as if there was no ſuch thing as *principles of convenience*, or, if there were ſuch principles, that they were to be ſacrificed to mere ſpeculative truths (if truths) of much leſs importance: a truly harſh, intolerant, and deſpotic maxim! as ill calculated to point out the moſt advantageous road to ſcience, as the maxim that a ſtraight line ſhould always be followed, would be to inſure us the beſt road to the ſummit of a mountain, though preſenting in that direction a ſeries of ſcabrous and abrupt precipices; and hence departed from by Lavoiſier himſelf, as we have ſeen in the inſtances of *ſaltpetre* and *borax*, and indeed by the whole French ſchool in the inſtances of *water* and *diamond*, as already mentioned: for common ſenſe, in ſome inſtance or other, ſeldom fails of aſſerting its rights: yet Lavoiſier tells us he was cenſured (by ſome chemical bigots) for this condeſcenſion. The principles of religion and juſtice are the only that can in no poſſible caſe yield to conveniency.

Among many juſt reflections that occur in the preface to Lavoiſier's celebrated elementary treatiſe of chemiſtry, there are ſome connected with this ſubject that appear to me not quite correct. Thus, p. x. (of the original) he tells us, "that the only way of avoiding theſe errors (unfounded hypotheſes) is to ſuppreſs reaſoning, or at leaſt to ſimplify it as much as poſſible, as it proceeds from us, and can alone lead us aſtray; to try it always by the teſt of experiment, to preſerve only the facts, which are the *data* given by nature, and which cannot deceive us; to ſeek for truth only in the natural concatenation of experiments and obſervations, as mathematicians arrive at the ſolution of a problem by the ſimple arrangement of the *data*, &c." From this paragraph we might be led to conclude that all reaſoning ſhould be baniſhed from chemical inveſtigation, or, at leaſt, that only the ſimpleſt ſhould be admitted; yet it may eaſily be ſhown that

* Ann. Chem. XXV. p. 207.

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the moſt ſignal inſtances of ſucceſsful chemical inveſtigation in the obſcureſt ſubjects, and the happieſt diſplay of chemical ſagacity, are the reſults of very complicated reaſoning. Such is Berthollet's theory of aqua regia, Berthollet and Welter's obſervations on hepatic air, Fourcroy's on hepatic waters, Vauquelin's theory of the mutual decompoſition of nitrous air and the ſolution of vitriol of iron *, moſt of Scheele's and many of Klaproth's analyſes, and a few others.

The juſt arrangement to which mathematicians owe the eaſy ſolution of their problems, is itſelf the reſult of profound reaſoning, as is evident in the formation of equations. But the modes of reaſoning employed in the ſolutions of mathematical and chemical problems cannot properly be compared, the former being founded on the relation of identity or equality, and the latter on that of cauſe and effect.

Page vi. he tells us, "it is the ſeries of facts that conſtitutes ſcience." I ſhould rather ſay, it was a knowledge of the relation that ſubſiſts between the facts that occur; but neither the facts themſelves can often be diſcovered without much ſubtile reaſoning, nor can they be marſhalled in a luminous ſeries without diſcovering the reciprocal relations of the component ingredients of compound ſubſtances to each other; a diſcovery which often requires an elaborate train of reaſoning.

Laſtly, both he and Morveau tell us that the memory of learners is ſingularly relieved by compound denominations expreſſing the component ingredients of each compound. In reply to which, I ſay, that the ſcience is not to be charged with a cumberſome train of words merely to gratify the indolence of beginners. Are we then, on every occaſion, to ſubſtitute the definition of words for the words themſelves? Are we, in imitation of the Germans, to ſay a *handſhoe* inſtead of a *glove?* Helvetius has long ſince remarked, that every man of common underſtanding poſſeſſes ſufficient power of memory

* All foreign chemiſts are infinitely obliged to him for giving the old denominations of weights and meaſures inſtead of centimetres, &c. Chemiſtry aims at enlightening the world, and not Frenchmen alone; it ſhould therefore ſpeak a language univerſally underſtood, and ſhake off the yoke of national pedantry.

to

to retain the ſignification of moſt words in his own, and often of thoſe of ſeveral other languages: chemiſtry and mineralogy together ſcarcely preſent two hundred appertaining to them alone; neither the ſciences of aſtronomy, law, or medicine, afford fewer.

The faſhionable rage of coining new words from the Greek, *without any neceſſity*, has been particularly baneful to mineralogy, inſomuch that foreigners, though well acquainted with the received terminology, cannot, without being verſed in that language, underſtand the meaning of the new-fangled terms lately introduced. It is well known that the mineralogical knowledge of all Europe is chiefly derived from the Germans and Swedes, whoſe nomenclature is in moſt inſtances the ſame, and where any ambiguity has ariſen it has been removed by the exertions of Werner. His nomenclature, where not too diſcordant with the language, or at open variance with the received technical names of other countries, ſhould therefore, for the ſake of preciſion and uniformity, be univerſally preſerved.

Abbé Haüy, who is now preparing a treatiſe of mineralogy, of which the higheſt expectations are juſtly entertained, ſince, in addition to his own ſuperior intelligence and profound phyſical knowledge, he is aſſiſted with the chemical abilities of Guyton, Vauquelin, Deſcotilles, and many others, and the extenſive reſearches of Dolomieu, ſeems convinced of the propriety of retaining the received terminology, at leaſt for the preſent, with the limitations and reſtrictions above mentioned; for, in his prefatory diſcourſe, Journal des Mines, XXVII. p. 224, he tells us, "that with reſpect to minerals of the firſt claſs (that is, conſiſting of mere earths), he left them thoſe names which they had hitherto borne, and did not take the liberty of impoſing new ones, except in caſes of neceſſity, as where a new ſpecies occurred, either formerly unknown, or confounded with one of a different nature. We have altered ſuch names only as were intolerably ambiguous."

Yet after this declaration we find the following new-coined names:

Teleſia, for oriental rubies, ſapphires, and topazes. Thus ſapphire is called *blue teleſia*, &c.—*Cymophane*, for the chry-

ſoberil of Werner.—*Amphibole* for cryſtalliſed hornblende.—*Pyroxene*, alſo cryſtalliſed hornblende; at leaſt I have reaſon to think ſo, for of the eleven external characters of both, given by La Metherie, there is only one in which they abſolutely differ, that is, the electrical.—*Staurotide* for ſtaurolite, the name very properly impoſed by La Metherie inſtead of the compound name it bore before.—*Axinite* I believe to be only a variety of thumerſtein.—*Actinote* for actinolite, the termination *lite* moſt properly denoting a ſtone.—*Thallite* for delphinite, the name already given it by Sauſſure.—*Idocraſe* ſeems, as I conjecture (ſeeing no exact account of it), a variety of olivin.

I paſs over ſeveral new names, as *euclaſe*, *dioptaſe*, *chabaſie*, &c. as they may poſſibly denote new ſpecies, which I wiſh were ſettled in conjunction with Berthout or Van Buch, or ſome of the Wernerian ſchool. Yet even theſe denominations, we are told, are only *proviſional*, being hereafter to be altered as analyſes may require.

I muſt here add, by way of note, that C. Guyton, in reviewing the firſt volume of my Mineralogy, Ann. Chem. p. 105 and 106, has fallen into two miſtakes, which I am perſuaded his candour will prompt him to acknowledge. The firſt is, in ſtating that my experiments on the fuſibility of different ſtones and earths were made in limeſtone veſſels, whereas they were in Heſſian crucibles. The ſecond is, in ſtating the alleged infuſibility of barytic earth and lime as the reſult of my experiments, whereas I expreſsly quoted Lavoiſier, having myſelf made no experiments on ſuch mixtures. (See Mem. Par. 1783, p. 599 and 600.) Other writers have frequently imputed to me miſtakes of Bergman, though I expreſsly quoted him.

III. *A brief Examination of the received Doctrines respecting Heat or Caloric. By* ALEXANDER TILLOCH. *Read before the Aſkeſian Society, December* 1799.

[Concluded from Page 126.]

I SHALL now endeavour to prove that the poſition which maintains that "heat, when in chemical union in bodies, cannot be made to manifeſt itſelf but by the action of ſome new chemical affinity, does not hold univerſally, and therefore may fairly be queſtioned.

Gaſes, as every one knows, are formed by the ſolution of certain baſes in caloric. (Whether light be alſo an ingredient, or whether light be a modification of heat, or *vice verſa*, affects not the preſent argument.) The baſe or baſes, and the caloric, are chemically united, and therefore the caloric, according to the received doctrine, is *latent* in the gas; *it is not cogniſable by any external ſign or organ of ſenſe*. I ſhall not ſtop to inſiſt here upon the large increaſe of volume acquired by the baſe ſo diſſolved—an increaſe cogniſable by our organs of viſion, and alſo tangible if the gas be received in a bladder, having already uſed a ſimilar argument when ſpeaking of the converſion of water into ſteam—but proceed to obſerve that this *chemically combined caloric* may be, partially at leaſt, ſeparated from its baſe *by means merely mechanical*.

To ſuppoſe that any body can by mechanical force be reduced into a ſmaller compaſs without any portion of its original ingredients being thereby driven out of the maſs, would be an abſurdity. When we attempt to compreſs in a metallic veſſel, of a known capacity, a quantity of any gas equal in volume to twice that capacity, we, by the mechanical force employed with that view, abſolutely cauſe a portion of one of the ingredients in the compoſition of the gas to percolate through the materials of which the veſſel is made (every ſubſtance in nature being pervious to heat). That ingredient, that ſubſtance, is heat. The veſſel in this caſe performs the office of a filter, ſuffering the heat to paſs, but keeping back the other ingredients; and the quantity of heat paſſed off in

this caſe will be, if denominated by its volume, *exactly of the bulk of the capacity of the veſſel.* If the veſſel has been charged with a quantity of gas equal in bulk to *three* times its own capacity, the heat forced out will be *equal in bulk to twice that capacity*; and ſo of any other proportion of charge.

Here, then, the ſame effect takes place as when we confine ſteam; the means are mechanical in both; the heat is ſqueezed out through the ſides of the veſſel by the force employed, as water is ſqueezed from a ſponge by preſſure applied to it. Was the water in the ſponge of a different nature before and after being expreſſed, firſt latent and then ſenſible? If it would be abſurd to ſay ſo, why ſhould we ſuppoſe heat, when diſlodged by ſimilar means, to have undergone any change as to its nature, and to have been *latent* before but *ſenſible* after? meaning thereby that its characteriſtic properties were different before and after the proceſs.

But have we any experiments that prove heat to be actually ſeparated by mechanical force from ſubſtances in which it is held to be *latent* or *in chemical union?* When the condenſer of an air-gun, or any other proper veſſel, is charged with more than one atmoſphere, the materials, as the proceſs goes on, become of a higher temperature than the ſurrounding bodies*, and heat is given off to them. This ſhows that heat is expreſſed from the contained air; and that, not being able to paſs freely, but forced to paſs more or leſs ſlowly, according to the conducting power of the materials of the veſſel, it is partially accumulated in the materials, and then from them paſſed off to ſurrounding bodies to reſtore equilibrium according to the general law.

If the wall of ſeparation, which forces the confined gas to remain charged with a ſmaller quantity of heat than its conſtitution would permit, but for the violence employed, be removed, what takes place? The gas inſtantly ſeizes upon a portion of the caloric employed in maintaining the common temperature of the ſurrounding bodies, ſuited to its own capacity in its unreſtrained ſtate, and in conſequence reſumes its original volume. This plainly appears to be the caſe,

* See Dr. Darwin's paper on this ſubject, Philoſophical Tranſactions for 1788.

for

for any ſubſtance placed before the cock when it is opened is reduced in temperature. Water ſo placed will even be converted into ice, in conſequence of a portion of its heat being violently ſeized upon by the gas, and more quickly taken into union with it than the privation experienced by the water is compenſated for by ſurrounding bodies in the paſſage of caloric from them to reſtore the equilibrium.

I am aware that it may be objected againſt this experiment, indeed I have heard the objection offered, that the friction of the forcing ſyringe muſt excite or accumulate a conſiderable portion of heat in the materials of which it is made; that the air, paſſing inſtantly from it into the condenſing ball, muſt neceſſarily carry a portion of the heat along with it, having no intermediate bodies to come in contact with; and that therefore we have no evidence of heat being in this caſe expreſſed from the compreſſed air.

Some force might be allowed to this objection, if the air, when liberated, had no effect in reducing the temperature of the bodies expoſed to it; but, as it has the power of taking from them a portion of their heat, and of reducing them below the common temperature, the objection falls to the ground: for, if the heat found to be accumulated in the materials of the condenſing ball was only a conſequence of its containing air, *made hot by paſſing from a cylinder heated by friction*, the air when liberated ſhould *blow hot*, and *raiſe* the temperaure of the bodies expoſed to the ſtream inſtead of *lowering* it, as we find it does. The reaſoning here is concluſive, but any perſon may alſo have the evidence of a direct experiment. Expoſe the ball of an air-gun, charged only with one atmoſphere, to the heat of a common fire till the temperature of the ball be raiſed a few degrees: the contained air will now be in a ſituation to expand itſelf as ſoon as the valve is opened; for we know that the air has been heated by the ball. Expoſe a thermometer to the aperture as the air eſcapes, and the thermometer will indicate an increaſed temperature.

I have heard ſome object to the inference from a condenſing ball being heated when charged, by queſtioning the fact, for "they have often applied their hand to it without

perceiving any such effect." It may be true that a *thick* ball of small capacity will not give any sensible indication of heat *to a hand* applied to it, or even to a sluggish thermometer, which can only touch it in one point; for, where *a large mass* of materials is ready to receive the heat as it is expressed from the air, the effect may not be immediately obvious: it would be a long time before a large piece of ordnance could be made sensibly hotter by applying the flame of a taper to it, but who would therefore deny that heat has passed into the metal? Instead of applying their hands to the condensing ball, let them, as before stated, place a thermometer in the stream of the air when liberated, and they may receive complete evidence that the air demands a portion of caloric to bring it into equilibrium with the surrounding bodies, which could not possibly be the case if it had not previously suffered some privation.

I shall here briefly state the results of an experiment undertaken for the express purpose of ascertaining whether there had been any mistake in the facts reported by others respecting the phænomena which accompany the compression of air *.

By means of a powerful syringe A, Plate VI. fig. 1. (23 inches long and 2 inside diameter) made fast to one end of a table and to the floor of the room, the vessel B, constructed of tinned copper, and made fast to the other end of the table, was charged with as much air as could be forced into it by the strength of one man. The temperature of the room was 59°. By the condensation of the air, the thermometer C, introduced into a tube soldered into the vessel B (and open at its exterior extremity), was raised seven degrees. On exposing the bulb of another thermometer to the orifice of the cock D, which was then opened to dis-

* For the execution of this experiment I am indebted to the London Philosophical Society. Having stated to that Society the fact, which I wished to see ascertained, the subject was taken up with that alacrity and zeal which characterises all its proceedings. The apparatus was prepared by Mr. Varley, experimenter to that institution, and the experiment was made in the presence of the Society on the 18th of November 1799.

charge

charge the air, that thermometer ſunk to nine degrees below the temperature of the room, or fixteen degrees below the point to which the other had been raiſed by the expreſſed heat.

It cannot be urged that in this experiment the air paſſed immediately from the ſyringe into the air-veſſel, for it had to traverſe a tube interpoſed between them of about four feet in length.

Now, when it is conſidered that the thermometer was in contact with the veſſel B in only one minute point (for the tube in which it was inſerted, and againſt the ſide of which the bulb reſted, was more than three times the diameter of the bulb), and that the whole ſurface of B was above 150 ſquare inches; and when it is alſo recollected that only a ſmall portion of the effect which the air iſſuing from D was capable of producing could manifeſt itſelf by means of the thermometer expoſed to its action (as the thermometer was obliged to be kept at ſome diſtance to prevent its being broken by the diſcharge of the air); ſome idea, though not an adequate one, may be formed of the maſs of heat that muſt have been thrown off from the whole ſurface of B in the proceſs of charging that veſſel *.

The

* Subſequent to the delivery of the preſent paper to the Society, I received Mr. Muſhet's intereſting communication, which was afterwards inſerted in the Philoſophical Magazine for February 1800. It contains ſome curious facts which confirm and illuſtrate the reſults obtained from this experiment. Speaking of the effects produced in the blaſt-furnace by the nature, compreſſion, and velocity of the air uſed, he obſerves that the compreſſion always occaſions an increaſe of temperature. On entering a blowing cylinder immediately after ſtopping the engine, he finds the thermometer riſe from 15° to 17½° higher than the ſurrounding atmoſphere. A thermometer held in the middle of the current of blaſt was found to be reduced below the common temperature as much as the cylinder was raiſed above it. In ſome caſes, when the common temperature is about 54°, the blaſt, as it iſſues, will ſink the thermometer 2° or 3° below the freezing point.

He ſtates another fact (ſee alſo Mr. Roebuck's paper, Philoſophical Magazine, Vol. VI. p. 324.), which completely does away every idea of any part of the effect being derived from friction. At ſome iron works air-vaults of from 60,000 to 70,000 cubical feet in content are employed for the

The results obtained by the preceding experiment, though only a small part of the effect could be appreciated, are sufficient to ascertain the fact it was meant to establish—that the doctrine which asserts heat to exist in a latent state in the atmosphere, and consequently to be not separable from it without a chemical decomposition of the air, cannot be true, otherwise mechanical pressure and chemical decomposition mean the same thing, which the advocates for the existence of heat in two distinct states will hardly maintain.

But the effect would be much more striking, and the quantity of heat could even be comparatively estimated by employing in the experiment such an apparatus as would enable us, in some measure, to intercept the heat as it passes off from the whole surface of the air-holder, and accumulate it in some liquid.

After thinking on various contrivances, with this view, a method of constructing such an apparatus occurred to me, a description of which, though I have not yet had time to carry it into execution, I beg leave to lay before the Society.

Description of a Gaso-Calorimeter.

The gas-holder A, fig. 2. of any capacity, say a quart, constructed of tinned copper or any other metal, well soldered and riveted at the joinings, should be made double, or, in other words, the vessel, properly called the gas-holder, should be covered with another of the same shape made of thin metal, (as tinned iron) in such a manner that the two may not touch each other except at the lower part, where they are fitted on, and soldered fast to one common collar. By this means a space is left between the exterior surface of the gas-holder and the interior of the cover, to be filled with a fluid for the purpose which shall immediately be stated. The interior vessel, which is to receive the gas, is open at the collar, and into this collar is fitted the cock B, made of glass, because of the mercury to be employed in the experiments, and

the purpose of equalising the blast. In these the increase of temperature is more sensible than in the blowing cylinder. It takes place at a considerable distance from any mechanical friction, and is therefore evidently produced by heat extricated from the air.

for

for the convenience of ſeeing. The exterior veſſel has an opening at the top C, into which is fitted a glaſs tube CD. Through this tube (or before fixing the tube in its place) fill the ſpace between the interior and exterior veſſel with any coloured liquid.

If this double veſſel be now filled with, and immerſed in, warm water, it is plain that the heat thus communicated to it will expand the coloured liquid contained between the interior and exterior ſurfaces, and make it riſe to ſuch a height in the glaſs tube as ſhall correſpond to the temperature of the water in which it is immerſed. By employing water of different temperatures the different degrees of heat, as aſcertained by an accurate thermometer, may be marked upon the tube, or on a ſcale attached to it; and thus the veſſel itſelf will, in fact, become a large hollow thermometer.

To the lower end of the cock B attach another veſſel E, made of caſt iron, furniſhed with two other cocks, *a* and *b*, and connected with the vertical tube FG, made of iron (a ſeries of gun barrels properly joined to each other) or of glaſs, ſecured by a caſe of wood, with cement interpoſed between it and the glaſs.

Provide alſo a veſſel of the ſame capacity as the internal dimenſions of the gas-holder A. This veſſel may be called the meaſure.

From the above ſhort deſcription the conſtruction of the gaſo-calorimeter, its uſe, and the way of operating with it, may be eaſily conceived.

Open the cocks *a* and F, and pour mercury into the funnel at G. It will riſe in E, driving the air before it, which will eſcape by the cock *a*. Having in this way filled the veſſel E with mercury up to the cock B, ſhut the cock F, place the meaſure before mentioned below the cock *b*, and by that cock draw off one meaſure of the mercury. It is obvious that by ſo doing one meaſure of air will at the ſame time paſs into E through the cock *a* *. The cocks *a b* being now ſhut, pour mercury into the tube FG to any convenient height; open the cock B, and then the cock F. The mea-

* Any other gas may be introduced by connecting this cock with a pneumatic apparatus.

ſure of air contained in the veſſel E, having now no way to eſcape, will, by the aſcent of the mercury in E, be forced to paſs through the cock B into the gas-holder A. As ſoon as this is perceived, by the mercury ſhowing itſelf in the glaſs cock B, ſhut that cock.

The gas-holder will thus become charged with two atmoſpheres, and that in a way ſo expeditious as to allow but little time for the eſcape and loſs of caloric, eſpecially if the tube FG be of large diameter, and if the veſſel E have been previouſly lined, and the veſſel A covered over with ſome bad conductor of heat. The expreſſed heat muſt then paſs through the ſides of the gaſo-calorimeter, and there, exerciſing its action on the interpoſed thermometric fluid, muſt make it aſcend in the tube CD to a height proportioned to the heat thus communicated to the fluid *.

Another atmoſphere may be introduced by ſhutting the cock F, opening *a* and *b*, and withdrawing another meaſure of mercury as before, and then, by mercury poured into GF, making the air to aſcend into A, in the manner already deſcribed †. Then note again the height to which the thermometric fluid riſes in CD; and ſo of a fourth or a fifth atmoſphere; obſerving that the height of the column of mercury in FG muſt always be proportioned to the number of atmoſpheres to be introduced.

The quantity of coloured fluid contained between the exterior and interior of the double veſſel A, ſhould be determined by weighing the quantity introduced before graduating the tube CD. By this means it will be poſſible to determine not merely how many degrees the heat expreſſed from the air raiſes the fluid in the tube CD, but to how many ounces of the fluid it communicates that degree of temperature. The weight of the materials of which the double veſſel

* As mercury is found to be a good conductor of heat, it would be adviſable to have a little oil, or ſome other liquid, in the veſſel E, before the firſt introduction of the mercury. The oil would riſe on its ſurface, and the interpoſed ſtratum riſing with the mercury prevent its contact with the air driven before it into the veſſel A.

† Or two or more atmoſpheres may be forced up at once, if the veſſel E be made of ſufficient capacity.

is

is made, ſhould be alſo determined, that it may be known how many ounces or pounds of metal have alſo been heated to the ſame degree.

The converſe may be exhibited by the ſame apparatus by a different mode of operating. Detach the veſſel A from the veſſel E; invert it, and fill it with water, previouſly deprived of air by boiling or by the air-pump, and, having ſhut the cock B, again invert it and join it to the veſſel E. Fill the latter with mercury in the manner that has been already deſcribed, and to the cock *b* adapt an iron tube, of 31 or 32 inches in length, to deſcend vertically into a baſon of mercury. If the cocks B *b* be now opened, all the others being ſhut, mercury will deſcend from E, through the tube fitted to *b*, into the baſon in which the lower extremity of the tube is, and the water will paſs from A into E, leaving a vacuum in A. When the water has all paſſed out of A, ſhut the cock B.

If one atmoſphere be now introduced into E, and, the cocks *a* and *b* being ſhut, be compreſſed into a half, a fourth, or a fifth of its original volume, by means of a proportionate column of mercury in the tube FG, heat will be expreſſed from the air, which muſt paſs off through the ſides of the veſſel E. Allow the apparatus to ſtand for ſome hours, till every thing connected with it has come to the common temperature. Then open the cock B, and the compreſſed air, finding itſelf at liberty, will paſs up into the vacuum in A, where it will find ſpace to expand itſelf to its natural volume, the quantity employed being one meaſure, or, in other words, equal exactly to the capacity of A. The mercury ſhould alſo be allowed, by opening the cock F, to aſcend till it reach the cock B. But the air, in reſuming its original volume, will demand heat from the materials of which A is conſtructed, and from the thermometric fluid of A, faſter than they can receive it from the atmoſphere and contiguous bodies, eſpecially if defended by a covering made of a bad conducting ſubſtance; and, as a neceſſary conſequence of this, the thermometric fluid will fall in the tube FG.

If heat be thus detected paſſing from or into air by mere

preſſure, or the contrary, and that it will we have already ſufficient evidence, *of what kind is the heat?* According to the received doctrine, it is neither *ſenſible* nor *latent* heat; for that heat which, united to oxygen and azot, forms atmoſpheric air, is *not ſenſible*, and that which raiſes the temperature of bodies is *not latent:* but this heat *conſtitutes* one-half, two-thirds, three-fourths, or four-fifths *of atmoſpheric air* (according to the number of atmoſpheres compreſſed into one), is a conſtituent *chemical* ingredient of the atmoſphere, and therefore *latent heat:* and yet it raiſes the temperature of other bodies, without being ſeparated from its union with the oxygen and azot *by the exerciſe of any chemical affinity:* therefore it is *ſenſible heat.*

The doctrine ſeems evidently to ſtand oppoſed to itſelf; or ſhall we, to avoid this concluſion, ſay that heat has a third mode of exiſtence, in which it may be called SENSIBLE-LATENT CALORIC! Abſurd as ſuch a poſition might be, it would not be more ſo than the doctrine of latent heat, taking that term in its common acceptation.

I meant to have troubled the Society with a few further remarks on this intereſting ſubject, but, having already encroached further upon its time than I at firſt intended, muſt defer them for the preſent.

To conclude: The doctrine of latent and ſenſible heat appears to me to have ariſen from a want of due attention to the facts eſtabliſhed by the veteran Black reſpecting the different capacities of bodies for heat. This eminent chemiſt was the firſt who propoſed any thing rational on this ſubject, and, when the beautiful ſimplicity of the firſt principles which he eſtabliſhed is duly attended to, it appears wonderful to me that the ſimple and permanent ſtructure that might have been reared upon them ſhould have eſcaped his ſagacious obſervation. Having eſtabliſhed this incontrovertible fact, that *different* ſubſtances have different capacities for heat, this truth of itſelf neceſſarily embraced another, which, though it could not poſſibly eſcape obſervation, has never been applied as it ought; namely, that in every chemical combination we effect we are altering the capacities of bodies for heat, and conſequently deranging the equilibrium; for

the product *differs* in its capacity from the ingredients; and the ſame holds in decompoſitions. Had this been properly attended to, it would have been found perfectly ſufficient, when taken along with that property by which heat tends to equilibrium, to explain the paſſage of heat from ſubſtances to other ſubſtances, without ever once ſuppoſing the heat changed in its properties. Heat ſeems to act uniformly, and its effects depend always on its quantity (not kind) compared with the capacity of the body into which it enters: but it is continually bandied about, as it were, by the conſtant changes that are paſſing upon bodies, by which their capacities for receiving or holding it are altered; ſo that it is in a conſtant ſtate of influx and efflux in bodies, and there is going on a conſtant adjuſtment, as it were, of the differences exiſting among them, each requiring its own ſhare of the common ſtock, and giving off, receiving, or merely tranſmitting heat, according to circumſtances. Bodies are continually undergoing change by the action of heat. This is admitted on all hands. Is it neceſſary then to look for a change in the *agent* as well as the patient? In many reſpects its action, as we have before obſerved, may be illuſtrated by that of water. Different ſubſtances require different quantities of water to diſſolve them; and different ſubſtances require different quantities of heat to diſſolve them. The property which different ſubſtances have to take in different quantities of water, may be called their capacity for water: but who ever talks of a certain quantity of water, when diffuſed among any number of ſubſtances in proportion to their capacities, being *latent water?* or, when an interpoſed hygrometer is affected by its paſſage from one ſubſtance to another, of its being then *ſenſible* or *free?*

IV. *Reflections on the Theory of the Infinitesimal Calculus. By* C. CARNOT, *Ex-Director of the French Republic, Minister of War, and Member of the National Institute, Paris* 1797. *Translated from the French, and illustrated with Notes, by* WILLIAM DICKSON, *LL. D.*

To the Editor of the Philosophical Magazine.

DEAR SIR,

THE mathematical readers of your excellent publication need not be told of the importance of the infinitesimal calculus, or of the assistance it lends to every part of natural philosophy, which falls not within the province of chemistry. That calculus is practically the same with what we call the method of fluxions, except in name and notation. But the theories of the two methods are different; or, rather, those theories are only different ways of illustrating one and the same method under different names and symbols. The fluxionary theory, as delivered by its inventor, the incomparable Newton, is confessedly the most accurate; but the infinitesimal, otherwise called the differential theory, is generally thought, I know not for what reason, to be the most easily understood *. As it is often advantageous to consider a subject in different points of view, it was perhaps fortunate that M. Leibnitz claimed this discovery, as he was, of course, obliged to give it a new explanation; and thus we have the ideas of two of the most acute men who ever existed, on one of the most extraordinary and sublime speculations which ever exercised the human intellect.

* "We trust," said the excellent apostle, with infinitely more depth and propriety than appears at first sight; "we *trust* that we have a good conscience." I *trust* that I understand something of Newton's fluxionary theory; but I never could understand the differential, as delivered by Leibnitz, in the *Acta Eruditorum* of Leipsic, for October 1684, of which the learned and ingenious Mr. Raphson has inserted a translation in his History of Fluxions. I believe most impartial persons, who *try* to read that perplexed and perplexing piece, will be far from accusing the historian of severity, when he says, that it contains "far-fetched, symbolising, insignificant novelties," and that the notation is "less apt and more laborious" than that of the real inventor.

In

In the eſſay, of which the following is a tranſlation, the two theories are blended, and ſo clearly explained, that thoſe who think the *pure* fluxionary theory, even as facilitated by Mr. T. Simpſon, in his excellent "Select Mathematical Exerciſes," ſtill ſomewhat difficult, may derive inſtruction from the author's new and perſpicuous way of treating the ſubject. This theory, like other mathematical theories, being of an abſtract nature, may perhaps, after all, be more eaſily conceived than expreſſed. And, as happens in the practice of arithmetic, there may be perſons who are expert in the application of fluxions, but who have never been able fully to ſatisfy themſelves as to the demonſtrable validity of the theory. It ſhould however be remembered, that all rational practice entirely depends on ſound theory; and, to uſe the words of the great Maclaurin *, in proportion as the "general methods are valuable, it is important that they be eſtabliſhed above all exception; and ſince they ſave us ſo much time and labour, we may allow the more for illuſtrating the methods themſelves."

Thoſe who, with a competent knowledge of geometry, algebra, and the conic ſections, attentively peruſe the following eſſay, will perhaps wonder why a ſubject capable of ſuch clear illuſtration has been repreſented as ſo difficult. The performance, though obviouſly not without faults, is, upon the whole, well worthy of the attention of ſuch mathematicians as I have alluded to. The tranſlation is as literal as the idioms of the two languages would bear; and I have annexed notes where I thought they would be uſeful. Though I decidedly prefer the fluxionary notation, as well as theory, to the differential; yet in the *text* I have retained, among the other ſymbols of the original, all the *d*'s, and the *dd*'s, and the *ddd*'s and the *dddd*'s, to which the foreign mathematicians are ſo *partial*, as being moſt congenial with the ingenious author's manner of conſidering his ſubject.

I am,

Dear Sir,

Sincerely and reſpectfully yours,

W. D.

* In the Introduction to his Treatiſe of Fluxions.

Adver-

Advertiſement by the Author.

Some years ago, the author of the following reflections reduced them to the form in which he now preſents them to the public. He is at preſent charged with buſineſs ſo very important as to forbid his reſuming his former ſtudies; but, as every circumſtance announces that the mathematical ſciences are about to ſoar to a new elevation, it is believed that ſome advantage may reſult from the publication of a memoir in which the theory of the differential calculus is diſcuſſed at large, and with preciſion, and in which are united the different points of view in which that theory has been conſidered.

Subject of the following Diſſertation.

1. No diſcovery ever produced ſo happy and ſo ſudden a revolution in the mathematical ſciences as the infiniteſimal analyſis; nor hath any improvement furniſhed us with ſuch ſimple and efficacious methods of arriving at a knowledge of the laws of nature. By decompoſing, ſo to ſpeak, magnitudes into their conſtituent elements, that analyſis ſeems, as it were, to have detected their internal ſtructure and organiſation. But as all extremes elude the cogniſance of the ſenſes and the powers of the imagination, we have been able to form but an imperfect idea of thoſe elements, a ſingular kind of beings, which muſt ſometimes be treated as real quantities, ſometimes as abſolute nullities, and which ſeem, by their equivocal properties, to hold a middle place between magnitude and zero, between exiſtence and nothing *.

Happily this difficulty hath not interrupted the progreſs of the diſcovery. There are certain primitive ideas which are always ſomewhat clouded in the imagination, but whoſe firſt

* I here ſpeak in conformity with the vague ideas which we commonly entertain of infiniteſimal quantities, when we have not been at the pains to examine their nature. But in truth, nothing can be more ſimple than the notion of infiniteſimals. Indeed, to ſay that a quantity is infinitely ſmall, is preciſely to affirm, that it is the difference of two magnitudes which have for their limit the ſame third magnitude, and nothing more is meant. The idea, then, of an infiniteſimal quantity is not more difficult to conceive than that of a limit; but it has beſides, as is univerſally allowed, the advantage of leading to a much more ſimple theory.

conſequences,

consequences, when once fairly deduced, open a vast field, which may be easily traversed. Such appears to be the idea of infinity, of which many geometricians, who *perhaps* were never able to fathom it, have made the most happy use *. Philosophers, however, unable to content themselves with so vague an idea, have been wishful to ascend to principles; but in doing this, they soon found themselves divided in opinion, or rather in their manner of viewing the objects of their research. My design in this performance is to reconcile those different points of view, to show my readers their relations, and to propose new ones; and I shall think myself well rewarded for my trouble, if I should succeed in throwing some degree of light on a subject so interesting.

The Origin which the Infinitesimal Calculus might have had.

2. The difficulty frequently experienced in accurately expressing by equations the different conditions of a problem, and in resolving those equations, *might* have given birth to the first ideas of the infinitesimal calculus. When, indeed, it is too difficult to give an exact solution of a question, it is natural for the analyst to seek the means of approximating to it as nearly as possible, by neglecting the quantities which embarrass the combinations, if it be foreseen that such quantities, on account of their small value, may be neglected, without producing any sensible error in the result of the calculation. Thus, for example, as the discovery of the properties of curve lines is very difficult, they have been considered as polygons

* Certainly no geometrician, or metaphysician either, was ever able to fathom quantities infinitely great or infinitely small; I had almost said, even in the restricted sense of the mathematicians, who define them to be quantities greater (or less) than any assignable quantities. Yet the metaphysicians can prove that such quantities must necessarily be received among the other *entia rationis*; and the mathematicians demonstrate many of their relative powers and properties; just as the chemists, *mutatis mutandis*, exhibit to the senses many of the properties of natural bodies, though they do not pretend to have any idea whatever of their intimate essences. On the difficulty, or rather impossibility, of conceiving, and the necessity of admitting, Eternity and Infinity, see Dr. Samuel Clarke's excellent Demonstration of the Being and Attributes of God, page 8, *et seq.* 10th edition. W. D.

of a very great number of ſides. If, indeed, we conceive a regular polygon to be inſcribed in a circle, it is evident that thoſe figures, though they be always different, and can never become identical, yet reſemble each other more and more as the ſides of the polygon increaſe in number. The circumferences of thoſe figures, their ſurfaces, the ſolids formed by their revolutions about a given axis, the analogous lines drawn without and within them, the angles formed by thoſe lines, &c. are, if not reſpectively equal, at leaſt the nearer to equality the more the number of ſides augments. Hence, by ſuppoſing the number of ſides to be very great, we may, without any ſenſible error, attribute to the circumſcribing circle, the properties which we have diſcovered to belong to the inſcribed polygon.

Farther, the ſides of this polygon evidently diminiſh in magnitude in proportion as their number is augmented; and conſequently, if we ſuppoſe the polygon to be really compoſed of a very great number of ſides, we may alſo affirm that each of thoſe ſides is really very ſmall.

This being underſtood, if, in the courſe of calculation, a caſe happen to occur in which many operations would be much ſimplified by neglecting, for example, one of thoſe ſides, which is ſmall in compariſon with a given line; that is, by employing in the calculation that given line inſtead of a quantity equal to the ſum of that given line and the little ſide in queſtion, it is clear that this may be done without inconvenience; for the reſulting error muſt be extremely ſmall, and it will not be worth while to inquire into its value.

3. For example, if it were propoſed to draw a tangent to a given point M, of the circumference of the circle MBD (fig. 1).

Let C be the centre of the circle, and DCB the axis. Suppoſe the abſciſs $DP=x$, the correſponding ordinate $MP=y$, and let TP repreſent the ſubtangent required.

In order to find that ſubtangent, let us conſider the circle as a polygon of a very great number of ſides. Let MN be one of thoſe ſides, and be produced till it meet the axis. It will then evidently be the tangent in queſtion, becauſe it falls not within the polygon. Moreover, let fall the perpendicular

pendicular MO upon NQ, which is parallel to MP, and put a for the radius of the circle. We shall then evidently have

$$MO : NO :: TP : MP; \text{ that is, } \frac{MO}{NO} = \frac{TP}{y}.$$

On the other hand, the equation of the curve for the point M being $yy = 2ax - xx$*, the equation for the point N will be

$$(y + NO)^2 = 2a\,(x + MO) - (x + MO)^2;$$

and subtracting the former equation from the latter, and reducing, we have

$$\frac{MO}{NO} = \frac{2y + NO}{2a - 2x - MO}; \text{ but we before found}$$

$$\frac{MO}{NO} = \frac{TP}{y}; \text{ therefore } \frac{TP}{y} = \frac{2y + NO}{2a - 2x - MO},$$

and multiplying by y, we have $TP = \frac{y\,(2y + NO)}{2a - 2x - MO}$.

If, then, MO and NO were known, we should have the value of TP, the subtangent required. Now the quantities MO and NO are very small, being each of them less than the side MN, which, by the supposition, is itself very small. We may therefore, without any sensible error, reject those quantities, in comparison with the quantities $2y$ and $2x - 2a$, wherewith they are connected. The equation will then be reduced to $TP = \frac{2y^2}{2a - 2x} = \frac{y^2}{a - x}$, the value of the subtangent sought.

The Infinitesimal Calculus may, in this View, be considered as a simple Method of Approximation.

4. If this result be not absolutely exact, it is at least evident that, in practice, it may pass for such, because the quantities MO and NO are extremely small. But a person who has no idea of the doctrine of infinites, will perhaps be surprised to be told that the equation $TP = \frac{y^2}{a - x}$, does not barely make a near approach to the truth, but that it is really and perfectly accurate. Of this, however, he may easily satisfy himself by

* For, in the circle, $2a - x : y :: y : x$.

inveſtigating the value of TP, on the principle of the tangent being perpendicular to the extremity of the radius; for it is evident that the ſimilar triangles,

$$CPM,\ MPT,\ \text{give}\ CP : MP :: MP : TP;$$

and therefore $TP = \frac{MP^2}{CP} = \frac{y^2}{a-x}$, as before.

5. As a ſecond example, ſuppoſe we are to find the ſurface of a given circle. Let us ſtill conſider that curve as a regular polygon of a great number of ſides. The area of any regular polygon is equal to the product of its circumference into half the perpendicular let fall from the centre on one of its ſides. The circle, therefore, being conſidered as a regular polygon of a great number of ſides, its ſurface ought to be equal to the product of its circumference (that is, the ſum of very numerous ſides) into half the radius; a propoſition as exact as the other.

6. However vague and indeterminate the expreſſions *very great* and *very ſmall*, or others of the ſame nature, may appear, we ſee, by the two preceding examples, that they are not unuſefully employed in mathematical combinations, and that the quantities ſignified by theſe expreſſions may afford much help in facilitating the ſolution of divers queſtions which may be propoſed. For, the notion of ſuch quantities being once rightly underſtood and admitted, all other curves, as well as the circle, may be conſidered as polygons of a great number of ſides; all ſurfaces may be divided into a great number of fillets or zones; all bodies may be reſolved into corpuſcles; and, in a word, all quantities may be decompoſed into particles of the ſame ſpecies with themſelves. Hence ariſe new relations and combinations; and we may eaſily judge, from the examples already adduced, of the reſources in calculation, which the introduction of ſuch elementary quantities muſt afford.

7. But the advantages of admitting theſe quantities is ſtill more conſiderable than could, at firſt ſight, have been expected. For we have ſeen, in the examples propoſed, that what was conſidered, at the outſet, as a ſimple method of approximation, conducts us, at leaſt in certain caſes, to reſults perfectly accurate. It becomes intereſting, therefore,

to

to be able to distinguish the cases in which this accuracy takes place, and thus to convert this method of approximation into a calculus perfectly exact and rigorous; and such is the object of the infinitesimal analysis.

8. Let us first see, then, how it came to pass that, in the equation $TP = \frac{y(2y + NO)}{2a - 2x - MO}$, found in article 3d, though we neglected MO and NO, the justness of the result was not affected; or rather, how the result became exact from the suppression of those quantities, and why it was not exact before that suppression.

Now, the reason of what happened in the solution of the above problem will easily appear, on remarking that the hypothesis on which that solution is grounded, is false; it being absolutely impossible that a circle can be a *true* polygon, whatever be the number of its sides; so that there must result from that hypothesis an error of some kind in the equation $TP = \frac{y(2y + NO)}{2a - 2x - MO}$. But the result $TP = \frac{y^2}{a - x}$ being nevertheless exact, as was proved by the comparison of the two triangles CPM, MPT, we found that MO and NO might be neglected in the first equation. Indeed these quantities *ought* to be neglected, in order to correct the calculus, and to destroy the error which had arisen from the false hypothesis that assumed. The rejection of quantities of this nature is not barely allowable, but is absolutely necessary in such cases, as being the only manner of expressing accurately the conditions of the problem.

The Accuracy of such Results is owing to a Compensation of Errors.

9. The accurate result $TP = \frac{y^2}{a - x}$ has been obtained then, only in consequence of a compensation of errors; and this compensation may be rendered still more evident, by treating the example above given in a manner somewhat different, namely, by considering the circle as (what it really is) a true curve, and not a polygon.

For this purpose, from the point R (fig. 1.) taken arbitra-

rily at any distance from the point M, draw the line RS parallel to MP, and, through the points R and M, draw the secant RT'; then we shall evidently have $T'P : MP :: MZ : RZ$, and therefore $T'P$, or $TP + T'T = MP \frac{MZ}{RZ}$. Now, if we imagine RS, moving parallel to itself, to approach continually to MP, it is evident that the point T' will, at the same time, more and more approach to the point T; and consequently, that we may render the line $T'T$ as small as we please, without affecting the proportion above established. If, then, we neglect the quantity $T'T$ in the equation just found, there will indeed be an error in the resulting equation $TP = MP \frac{MZ}{RZ}$, to which the former will now be reduced: but this error may be diminished as much as we please, by making RS approach to MP as nearly as is necessary; that is, the proportion of the two members of this last equation will differ as little as we please, from the proportion of equality.

In like manner we have $\frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ}$. (See Article 3.) And this equation is perfectly accurate, whatever be the position of the point R; that is, whatever be the values of MZ and RZ. But, the nearer RS approaches to MP, the smaller will be the lines MZ and RZ; and hence, if they be neglected in the second member of that equation, the error which will result in the equation $\frac{MZ}{RZ} = \frac{y}{a - x}$, will, as in the former case, be thereby rendered as small as we think proper.

This being so, without paying any regard to errors, which I have it always in my power to diminish as much as I please, I treat the two equations $TP = MP \frac{MZ}{RZ}$, and $\frac{MZ}{RZ} = \frac{y}{a - x}$, as if both of them were perfectly accurate: substituting, therefore, in the former the value of $\frac{MZ}{RZ}$, expressed in the latter, I have $TP = MP \frac{y}{a - x}$; that is, $TP = \frac{y^2}{a - x}$, as before.

This

This result is perfectly just, because it is conformable to that which was obtained by the comparison of the triangles *CPM* and *MPT*; and yet the equations $TP = MP\,\frac{MZ}{RZ} = y\,\frac{MZ}{RZ}$ and $\frac{MZ}{RZ} = \frac{y}{a-x}$, are certainly both of them *false*. For the distance of *RS* from *MP*, was not supposed nothing, or even very small, but equal to a line arbitrarily assumed. It follows, therefore, of necessity, that a mutual compensation of errors took place in the comparison of the two erroneous equations*.

Why this Compensation takes place.

10. Having shown that there exist compensating errors, whence they originated and how they are proved, I shall next proceed to explain them, and to search for a mark by which it may be known that a compensation takes place in calculations similar to the preceding, and the means of producing such compensation in particular cases.

* This "compensation of errors" the acute Dr. Berkeley, Bishop of Cloyne, in *The Analyst*, published in 1734, laboured to magnify into a serious objection against the differential calculus; not considering that errors which we may make as little as we please, will affect the truth as little as we please. Thus 1·999999, &c. is not, in rigid strictness, equal to 2; but as, by annexing 9's, the error may soon be rendered *incomprehensibly* minute, no one can doubt that its *ultimate ratio* to 2, is a *ratio of equality*. Hence the word *false*, applied by our ingenious author, in this paragraph and elsewhere, to differential equations, is abundantly too strong; and, had he read The Analyst, he would certainly have used a less exceptionable term, such as incorrect, or inaccurate, or *imperfect*, a word which in the sequel he takes in a similar sense. But *false* (*faux*, *fausse*) signifies not minuteness of error, but a total negation of truth. It is not always easy, indeed, to find words exactly suited to express very abstract ideas. The great Simpson (*Select Exercises*, p. 239.) admits that, had Newton used the word *limiting* instead of *ultimate* ratios, Dr. Berkeley might not have conjured up his "Ghosts of departed Quantities." Yet Mr. Simpson, and all other competent judges, allow that Newton, by representing fluxions not as in the ratio of the whole increments, but in the first ratio of those increments considered as nascent, or in their ultimate ratio considered as evanescent, has effectually anticipated every reasonable objection; and this, no doubt, the learned and ingenious bishop would have confessed, had he given himself time to digest this doctrine, which is partly unfolded in the present tract. W. D.

For

For this purpose, it will be sufficient to remark, that the errors, included in the two equations $TP = y\frac{MZ}{RZ}$, and $\frac{MZ}{RZ} = -\frac{y}{a-x}$, may be rendered as small as we please; and the same thing would take place if there were any error in the resulting equation $TP = -\frac{y^2}{a-x}$, and if that error depended on the arbitrary distance of the lines MP and RS. But no such error can exist in this last equation; because the point M, through which the tangent must pass, is given, and not one of the quantities, TP, a, x, or y, is arbitrary.

Hence it follows, that the compensation of errors, which takes place in the equations $TP = y\frac{MZ}{RZ}$, and $\frac{MZ}{RZ} = -\frac{y}{a-x}$, is indicated in the result by the absence of the quantities MZ and RZ, which caused those errors. Consequently, after having introduced these last quantities into the calculation, to facilitate the expression of the conditions of the problem, and having treated them, in the equations expressing those conditions, as nothing in comparison of the proposed quantities, we have only to eliminate those quantities out of the equations, in order to free them from error, and to obtain an accurate result.

11. The inventor of this method might therefore have been conducted to his discovery by a very simple train of reasoning. "If, instead of a proposed quantity," might he have said, "I substitute in my calculation a quantity which is not equal to it, some error will certainly result. But if the difference of the quantities in question be arbitrary, and I have it in my power to render that difference as small as I please, the error cannot be dangerous. I may even commit several similar errors at once, without incurring any inconvenience; because I have always in my own power the degree of precision, which I choose to give to my results. Besides, it may happen that those errors will mutually compensate each other, and, if so, my results will become perfectly accurate." But how is such a compensation to be effected in all cases? A little reflection will

will suggest an answer to this question. "In fact," might the inventor continue, "let me suppose for a moment that the desired compensation has taken place, and let me inquire by what mark it ought to be indicated, in the conclusion of the calculation. Now it must naturally happen, that when the quantities which occasion the errors disappear, the errors themselves will disappear at the same time. For those quantities (such as MZ and RZ) having, by the supposition, arbitrary values, ought no longer to exist in *formulæ*, or results, which are not arbitrary, and which depend, not on the will of the calculator, but on the nature of the things whose relation, expressed by those results, he proposed to investigate. The mark, then, which announces that the desired compensation has taken place, is the absence of the arbitrary quantities which produced the errors; and therefore I have nothing more to do, in order to effect that compensation, than to eliminate those arbitrary quantities."

12. With a view to fix those ideas the more firmly in the mind, and to give to the principles thence derived the necessary degree of precision and generality, I shall remark, that the quantities which we have occasion to consider in the subject before us, may be distinguished into two classes. The first class consists of quantities which are either given, or determined by the conditions of the problem, such as MC, MP, PT, MT. The second class is composed of quantities, such as RS, RT', ST', which depend on the arbitrary position of the point R, and which, as the point R approaches to the point M, do respectively approach their corresponding quantities in the first class. Thus MP, for example, is the limit of RS, that is, the fixed term to which it continually approaches, or, if you will, its last or *ultimate value*. In like manner, MT is the limit or ultimate value of RT', and PT that of ST'. For the same reason, it is clear that the limits or ultimate values of MZ, RZ, MR, $T'T$, are, every one of them, 0. In fine, it is also evident that the *ultimate* ratio of RS to MP, (that is, the ultimate value of $\frac{RS}{MP}$,) is the ratio of equality, and such is the ratio of RT' to MT, of ST' to PT, or, in a word, such is that of any other quantity to its limit.

13. To enable us to extend these remarks to other problems of the same kind, let us now put the case, That any system whatever of quantities is proposed, in order that the relations which exist between them may be investigated *.

14. And first, I shall comprehend under the name of *assigned* quantities *(quantités désignées)* not only all the quantities proposed in the very enunciation of a problem, but also all those which depend on those quantities, that is, which are functions of those quantities, and of none else.

15. On the other hand, I shall give the names of *unassigned*, or *auxiliary* quantities, to all those which make no part of the system of assigned quantities, and which consequently do not essentially enter into the calculation, but are introduced solely to facilitate the comparison of proposed quantities.

Thus, in the preceding example, MP, MC, MT, DP, &c. are *assigned* quantities; because they depend only on the position of the point M, to which the tangent is to be drawn. But RS, and all its dependent lines, MZ, RZ, $T'T$, $T'P$, &c. are *auxiliary* quantities; because we should not have thought of drawing them, except their assistance had been wanted in the solution of the question, the object of which is to discover the proportion, or relation, of MP to MT.

* I here suppose that, in any problem proposed, the relations which exist between such or such proposed quantities has been previously discovered. If, for example, the problem be, to find a curve which has a certain determinate property, I suppose that the relation between an ordinate of that curve and the corresponding abscisse is already known. In like manner, if it were required to draw a tangent to any indeterminate point of that curve, I begin by arbitrarily fixing on the point to which I wish to draw the tangent, and I limit the problem to the investigation of the relation which subsists, for instance, between the subtangent and the abscisse, or between the ordinate and the corresponding subnormal at that point. But if it should be asked how I apply the definition of *infinity* to such questions as these: *Is matter divisible* ad infinitum? *Is the space in which all created beings exist infinite?* I say, if such questions should be put to me, I should answer, that my definition is restricted to mathematical infinity, and that it can only be applied to problems whose object is the discovery of the relations which exist between different mathematical quantities, and that the metaphysical questions just stated can by no means be referred to the theory, of which I here propose to establish the principles.

Hence

Hence it evidently follows, that into all unassigned quantities there necessarily enters something arbitrary; for, if this were not the case, their value would then be assigned, by the very conditions of the problem, and consequently they would wholly depend on proposed quantities; which is contrary to the supposition.

16. Since in the mathematics, two lines, two surfaces, two solids, in short two quantities of any kind whatever, are supposed continually to approach each other by insensible degrees; so that their proportion, or the quotient representing that proportion, differs less and less, and as little as we please, from unity, we say, that the last, or ultimate ratio of those two quantities is a ratio of equality.

17. If one of the quantities be assigned, and the other auxiliary, the first is called the *limit*, or *ultimate value* of the second. Thus a limit is nothing else than an assigned quantity, to which an auxiliary quantity is supposed continually to approach, in such a manner that their difference may be rendered as small as we please, and that their ultimate ratio may be a ratio of equality.

Thus auxiliary quantities alone can be properly said to have limits; for assigned quantities, being supposed constant and unchangeable, and to be themselves the terms, or ultimate values of auxiliary quantities, cannot, in strictness, be said to have any limits; unless we choose to say, that every assigned quantity is its own limit, a liberty of speech which cannot be refused us; since the ultimate value of any determinate quantity whatever can be nothing else than that quantity itself.

18. Hence, in general, we give the names of ultimate values and ultimate ratios of quantities, to the values and ratios which are, in fact, the last, or ultimate, ones assigned to those quantities and their relations, by the law of continuity, when each of them is supposed to approach, by continued and insensible degrees, to its corresponding assigned quantity.

19. We generally give the name of an *Infinitely small* quantity, to the difference between any auxiliary quantity whatever and its limit. Thus, for instance, RZ, which is

the difference between *RS* and *MP*, is what we call an infinitely ſmall quantity.

20. On the other hand, we call every quantity *Infinitely great*, or ſimply *Infinite*, which is equal to unity divided by an infinitely ſmall quantity: conſequently the quantity $\frac{1}{RZ}$ or $\frac{1}{RS-MP}$ is infinite, or infinitely great.

But, ſince *MP* is the limit, or ultimate value, of *RS*, it is evident that 0 is the ultimate value of *RZ*, or *RS—MP*, and that $\frac{1}{0}$ is the ultimate value of $\frac{1}{RZ}$.

21. Thus we may affirm, in general, that *An infinitely ſmall quantity is a quantity whoſe limit is* 0; and on the contrary, that *An infinitely great, or ſimply an infinite, quantity is a quantity whoſe limit is* $\frac{1}{0}$.

22. Under the name of *Infiniteſimal quantities*, we comprehend quantities which are infinite, or infinitely great, as well as thoſe which are infinitely ſmall. All other quantities are called *finite*.

23. To ſay, in conformity with common uſage, that Infinity is that which has no bounds, or is that which is without limit, or is that whoſe limit does not exiſt, is a mode of expreſſion which conveys a ſimple idea of what is meant, and which is by no means deſtitute of propriety; becauſe in fact, the limit of one kind of infiniteſimal quantities is 0, and that of the other $\frac{1}{0}$; *and* 0 *and* $\frac{1}{0}$ *are not real quantities* *.

24. But,

* This kind of notation, which is in general uſe, is very apt and ſignificant. But we are under no neceſſity of making 0 always repreſent *abſolute nothing*. We are at liberty, when our ſubject requires it, to make 0 ſtand for *relative nothing*, or that which may be conſidered as nothing relatively to ſome other quantity. Thus any very ſmall fraction ·000000000001, (prefixing cyphers at pleaſure) though not abſolutely nothing, may be ſafely conſidered as nothing, *relatively* to, or compared with 1, and, *à fortiori*, with any other whole number, and accordingly may be repreſented by 0. And, if ſo, $\frac{1}{\cdot 000000000001}$, or 1000000000000 (annexing cyphers at pleaſure) though by no means abſolutely infinite, may be accounted infinite relatively to 1, or any moderate given number, and accordingly may be repreſented by $\frac{1}{0}$.—Thus the diameter of the earth

24. But, from 0 and $\frac{1}{0}$ being the limits of infinitesimal quantities, it by no means follows that these quantities themselves are chimerical entities. On the contrary, it appears from the definition (article 19) that an infinitely small quantity is the difference between two very efficient quantities, namely, between any auxiliary quantity whatsoever and its limit.

25. Hence it follows farther, that every infinitely small quantity may be considered as the difference between two auxiliary quantities, which have the same third assigned quantity for their limit. For, let X and Y be two different auxiliary quantities, whose limit is the same third quantity A. I say, that $X-Y$ is an infinitely small quantity. For, since the limit, or ultimate value of X is A, and that of Y is also A, it follows that the ultimate value of $X-Y$ will be $A-A$, or 0. Therefore the limit of $A+(X-Y)$ is A;

earth may be reckoned *very small*, compared with the distance of the sun, and as *relative* 0, compared with that of any fixed star; and conversely, the distance of the sun is *very great*, and that of a fixed star $\frac{1}{0}$, or infinite, compared with the earth's diameter.—Again; supposing a rectangle to be continually diminished by the parallel motion of one of its sides, the law of continuity tells us that it will come to 0, or that the moving side will arrive at the fixed side of the rectangle; so that 0 here is not absolute nothing, but it is a line, which however may be looked upon as 0, if compared with the rectangle; and the rectangle as $\frac{1}{0}$, or infinite, if compared with the line. In like manner, a point and a rectangle are as 0, or nothing, when respectively compared with a line and a parallelopiped; and these last as $\frac{1}{0}$, or infinite, when respectively compared with the two first. Thus we see that the sense of the 0 is always to be determined by the subject into which it is introduced. See Emerson's P. S. to the 2d edition of his Algebra, where he replies to the Reviewers, who had misunderstood the very elegant solutions of his 73d and 74th problems.

The 1st, 3d, 6th, and 7th corollaries to the former of these problems will be useful in this place.

"Cor. 1. If 0 multiply any finite quantity, the product will be nothing.

"Cor. 3. If a finite quantity be divided by 0, the quotient is infinite.

"Cor. 6. Adding or subtracting any finite quantities to or from an infinite quantity, makes no alteration.

"Cor. 7. Therefore, in any equation where are some quantities infinitely less than others, they may be thrown out of the equation."

W. D.

and

and consequently we may consider $X - Y$ as the difference between an auxiliary quantity $A + (X - Y)$ and its limit A, and this difference (by article 19.) is an infinitely small quantity. It may therefore be affirmed, in general, that *An infinitely small quantity is the difference between two auxiliary quantities which have the same limit.*

26. Two quantities cannot have the same third quantity for their limit, unless the ultimate ratio between those two quantities themselves be a ratio of equality. For, since, by the supposition, the limit, or ultimate value of $\frac{X}{A}$ is 1, and that of $\frac{Y}{A}$ is also 1, it is evident that the limit, or ultimate value of $\frac{\left(\frac{X}{A}\right)}{\left(\frac{Y}{A}\right)}$ is likewise unity. Now $\frac{\left(\frac{X}{A}\right)}{\left(\frac{Y}{A}\right)} = \frac{X}{Y}$; and therefore the limit, or ultimate value of $\frac{X}{Y}$ is 1; that is, the ultimate ratio of X to Y is a ratio of equality. It may therefore be affirmed, in general, That *An infinitely small quantity is the ratio which the difference of two quantities, ultimately in the ratio of equality, has for each of these two quantities.*

27. In fine, it may evidently be farther affirmed, That *An infinitely small quantity is an unassigned quantity, to which is at first attributed any arbitrary value whatever, and which is afterwards supposed to decrease insensibly till it arrive at* 0, *or zero.* Thus, in general, when it is said, *Let* Z, *for example, be an infinitely small quantity*, it is precisely tantamount to saying, *Let* Z *be any arbitrary quantity whatever*, (and consequently auxiliary, for assigned quantities cannot be arbitrary) *and let us afterwards suppose that quantity* (Z) *to decrease continually till it come to* 0, *or zero* *.

28. A quantity is said to be infinitely small *relatively* to another quantity, when the ratio of the first to the second is

* In the 25th, 26th and 27th articles. the author gives us different ideas of an infinitely small quantity; but which, if duly considered, will be found to amount nearly to the same thing. W. D.

an

an infinitely small quantity; and, reciprocally, the second is said to be infinite or infinitely great, *relatively* to the first.

29. Two quantities are said *to differ infinitely little*, or *to be infinitely little different*, from one another, when the ratio of the one to the other differs from unity only by an infinitely small quantity; so that their ultimate ratio is a ratio of equality, and such evidently are RS and MP.

30. The *Infinitesimal Calculus* may be defined to be the art of discovering, by the help of quantities called Infinitesimals, the proportions or relations, whatever they may be, which subsist between the different parts of any system of quantities proposed.

These infinitesimal quantities being nothing more than auxiliary quantities, introduced into the calculation only to facilitate the expression of the conditions of any proposed problem, it is evident that they must necessarily be eliminated out of the calculation, in order to obtain the desired result, namely, the relations sought. Thus we may in some respects affirm, That every Infinitesimal Calculation is an *unfinished* calculation; for in fact, as soon as the auxiliary quantities, which are not essential, come to be eliminated, the calculation ceases to be infinitesimal, and in every thing resembles an ordinary algebraic result *.

To complete the explanation of the principal terms which relate to the theory of Infinites in general, it remains for me to state what I understand by an imperfect equation.

31. I call every equation *An imperfect equation*, whose two sides are unequal quantities, the difference however between them being infinitely small; or, which comes to the same thing, Every equation is imperfect, the two sides of which, though unequal, have for their ultimate ratio, the ratio of equality.

Thus, for example, the *false* equations, $TP = y\frac{MZ}{KZ}$ and $\frac{MZ}{KZ} = \frac{y}{a-x}$ (see article 9.) are what I call imperfect

* Every one knows, indeed, that a calculation, in which Infinitesimal quantities have any place, is not supposed to be finished, and that we never think the result accurate till all the infinitesimal quantities are entirely eliminated.

equations; becauſe the quantities, neglected in the accurate equations, whence they are derived, are infinitely ſmall quantities. On the theory, therefore, of this kind of equations is founded the ſolution of the queſtion before handled, and that of all queſtions of that ſort. For this reaſon it is, that I ſhall proceed to inveſtigate the principles of the theory of imperfect equations, which is the foundation of the Infinitefimal Calculus, or rather, which is itſelf that calculus.

[To be continued.]

V. *Account of* C. F. DAMBERGER'S *Travels through the interior Parts of Africa, from the Cape of Good Hope to Morocco* *.

THIS adventurous traveller, a native of Germany, and brought up to the buſineſs of a carpenter, enliſted at Amſterdam, in the year 1781, as a ſoldier in the ſervice of the Dutch Eaſt India Company, with whom he entered into an engagement for ſeven years. On the 21ſt of June he ſailed from the Texel, and on the 21ſt of September arrived at the Cape of Good Hope, where he was conveyed to the hoſpital along with eighty-four other ſick, who had come out in the ſame veſſel. After being a month in the hoſpital, he was relieved from military duty, by being taken into the ſervice of Mr. Brand, the poſtmaſter at the Cape and preſident of the council, who ſent him to Falſe Bay, where he remained a whole year, employed in various labours, ſuch as loading and unloading ſhips, cutting down wood in the mountains, and other things of the like kind.

Soon after he was promoted by his maſter to be his houſeſteward, and in this ſituation diſcharged his duty for ſome time to the complete ſatisfaction of his employer, who afterwards carried him back with him to the Cape Town; but having quarrelled with his miſtreſs, he found himſelf expoſed to ſo much ill-treatment, that he reſolved to deſert, and to make his way to Europe by land. In order that he might be better able to carry this plan into execution, he requeſted his

* From C. F. Damberger's Landreiſe in das innere von Afrika, in den Jahren 1781 bis 1797. Leipzig, 1801.

maſter to ſend him back to Falſe Bay, where he expected he ſhould have an opportunity of effecting his eſcape with the leſs danger of detection. This requeſt was granted, and he ſet out for the Bay on the 14th of November 1783, after having put his box into the Company's waggon, received twelve months pay due to him, and diſpoſed of various articles which he conſidered as entirely uſeleſs for his intended expedition.

Among the Dutch troops at the Meiſenberge was a corporal named Martens, a native of Hanover, who having accompanied Colonel Gordon on a journey into the interior part of the country as far as Caffraria, had conſtructed a map of the diſtricts through which he had paſſed and preſented it to the Company, under an idea that he ſhould be rewarded for his trouble; but inſtead of a reward, he was enjoined, under the pain of ſevere puniſhment, never to attempt any thing of the like kind in future. This man however was ſo fond of theſe purſuits, that he ſtill amuſed himſelf with making charts, and our traveller after ſome difficulty prevailed on him to allow him to copy ſome of them in order to aſſiſt him in his enterpriſe.

On the 24th of December, every thing being ready for his journey, he obtained leave of abſence from the ſergeant on command under pretence of going to the Cape Town, but proceeded towards Stielenbuſch, ſo called from a governor of the name of Van Stiel, who formerly preſided over the Dutch ſettlements at the Cape, where he arrived next evening. Being acquainted with the bailiff of this farm, who had often ſeen him at Mr. Brand's houſe, he accoſted him with confidence, and was received in a very friendly manner, eſpecially as he told him that he was going to his maſter's plantation, to ſee that every thing was properly conducted.

At this place Damberger purchaſed a new fowling-piece, and on the 26th proceeded to the farm of another acquaintance, named Munch, from whom he obtained two pounds of powder and thirty balls. On the 27th he came in ſight of his maſter's farm, but did not approach it till the evening, leſt he ſhould be obſerved by the bailiff. As ſoon as it was dark he went privately into the apartment inhabited by the ſlaves,

whom he requeſted not to ſay any thing of his arrival, and, after partaking of ſome refreſhment, retired to reſt, but with a mind ſo agitated that he enjoyed very little ſleep. Next morning at day-break he continued his journey, and on the 19th of February reached Blettenberg's Bay, where he purchaſed ſeveral things neceſſary for his intended expedition, ſuch as a calabaſh to hold water, a few more pounds of powder, a flint and ſteel, matches, &c. During the few days that he remained here, he took up his lodging in the houſe of a perſon named Vogtmeyer, who was not then at home; but he was kindly entertained by his wife, and he embraced this opportunity of obtaining from the Hottentots in the family every information poſſible reſpecting the route he intended to purſue. The country around this place has a beautiful appearance, and is exceedingly fertile; it abounds with timber of every kind, and has a good ſupply of excellent water. When Vogtmeyer returned, which was on the 25th of February, he received our traveller with great friendſhip, but at the ſame time told him that he knew he was a deſerter, and that he would ſend him back to the Cape, where he was ſure he ſhould be well rewarded by his maſter. He however entertained no ſuch deſign; for, after being informed of Damberger's real views, he furniſhed him with a knapſack made of a calf's ſkin, a hand-bill, with ſeveral other ſmall articles, and wiſhed him a good journey.

Our traveller, who had now got to the diſtance of 74 miles from the Cape, again ſet out on the 26th of February, and, purſuing his way towards Caffraria, arrived next day at the firſt Hottentot kraal he had ſeen, which conſiſted of about twenty huts. In this kraal he remained nine days, at the end of which time he had an opportunity of continuing his journey in company with ſome Hottentots of another kraal, who had come hither to procure ſheep. Having proceeded two miles with great difficulty through the long graſs, which rendered their progreſs exceedingly tedious and tireſome, they halted for the night on the banks of the Silver River; but were unable to ſleep for fear of the wolves, which, attracted by the ſheep, kept prowling around them; as they were afraid of kindling fires to frighten them away, leſt they

ſhould

ſhould ſet fire to the graſs, which was exceedingly long and almoſt dry.

In the morning they reached the ſummit of the Milk Mountains, where they found good paſture; they therefore ſat down to breakfaſt, and turned their ſheep looſe to feed; but ſcarcely had they put a morſel to their mouth, when they eſpied ten men rapidly advancing towards them. Our traveller having ſoon learned from his companions that theſe ſtrangers were Boſhmen, who no doubt would attempt to plunder them of their ſheep, he endeavoured to inſpire them with courage, and, immediately loading his piece, fired it at the enemy, one of whom fell; the reſt then diſcharged their aſſagays, and betook themſelves to flight.

Next morning our traveller arrived at the kraal where his companions reſided. As he was much fatigued, having ſpent ſeveral nights without the leaſt repoſe, he threw himſelf down in one of the huts and enjoyed a ſound ſleep. When he awoke the Hottentots brought him ſome milk and roaſt mutton; and whilſt he was partaking of this repaſt the chief of the horde entered the hut in his dreſs of ceremony, and, bowing ſeveral times before him, thanked him for the aſſiſtance he had given to his countrymen. The chief then told him that theſe Boſhmen, a few days before, had ſtolen from the kraal thirty ſheep. Our traveller, in conſequence of this information, inquired what might be the ſtrength of the horde, and where they reſided. Being informed that they conſiſted of about thirty men, and that they reſided at a place in the mountains a few miles diſtant, where, to guard againſt the danger of an attack, they had fortified themſelves behind a kind of abbatis, and were furniſhed with a great number of ſtones, he told the Hottentots that he thought it not impoſſible to drive them from their lurking-place, and offered to lead the attack, if they would promiſe to ſupport him. The chief liſtened to this propoſal with great pleaſure, as it held forth hopes of recovering the ſtolen ſheep; but he ſtarted ſome objections as to the practicability of carrying it into execution. It was however at laſt agreed that the attempt ſhould be made, and Damberger, after giving proper inſtructions to the Hottentots, deſired each to furniſh himſelf

with a ſtout cudgel, a bundle of dry graſs, and a little dry wood. As ſoon as it grew dark, they proceeded in great ſilence towards the mountain, where, as ſoon as they arrived, our traveller divided the whole troop into two bodies, one under his own command, and the other under that of the chief. They then ruſhed towards the firſt fence, which being inſtantly ſet on fire, the flames were ſoon ſpread by the wind, and in a little while the whole mountain ſeemed to form one general conflagration. They now proceeded to a place at the bottom of the mountain, by which it was ſuppoſed the enemy would be obliged to make their eſcape. Here they had not long waited, when they heard a dreadful howling, and ſaw ſeveral of the Boſhmen half ſcorched ruſhing through the flames. To terrify them the more, and at the ſame time to give a more general ſignal to the Hottentots to attack them, our traveller diſcharged his piece among the flying enemy, who were attacked with ſuch fury by his Hottentot allies, that this whole horde of plunderers were extirpated. Next morning they not only recovered ſome of the ſtolen ſheep, but found ſeveral more which had belonged to the enemy. When the conquerors returned to the kraal, they were received with great joy. As our traveller found himſelf much fatigued by the expedition, he retired to enjoy ſome reſt in the chief's hut, but the Hottentots ſpent the whole night in rejoicing on account of their victory.

Damberger continued two weeks among theſe people, who treated him with great reſpect; but as it was then the moſt favourable period for travelling, he reſolved to proceed and to take advantage of the winter ſeaſon, as it is called, which takes place in April, and which however is diſtinguiſhed only by the weather being rainy and the air cooler.

In conſequence of this reſolution he left the kraal on the 25th of March, and directed his courſe eaſtwards towards Fiſh River, but with ſome caution to avoid falling in with any of the Dutch plantations, of which there were ſeveral in the neighbouring diſtricts. After paſſing another kraal he took up his lodging for the night in a third, which was the laſt within the juriſdiction of the Dutch Eaſt India Company. It was ſituated on the Fiſh River, at a place where it divides itſelf

itſelf into two arms, one of which flows between the mountains and the Dutch colonies, and the other into the Silver River already mentioned.

The author in ſpeaking of the Hottentots ſays, "Mr. Vaillant has given a very ample deſcription of the manners and cuſtoms of theſe people, but he is not always correct.

"The chief of a horde bears the title of *Montur*. This office however is not hereditary, and the perſons choſen to fill it are thoſe who have diſtinguiſhed themſelves by ſome heroic action, ſuch as killing ſome ſavage animal, or obtaining a victory over the enemy. The chief of each kraal, whether it lies within the juriſdiction of the Dutch or not, receives from the Company a large cane, having at the top a ſilver knob, inſcribed with the Company's arms, and which is borne by the chief on ſolemn occaſions. Each Hottentot alſo receives annually two pounds of tobacco and two bottles of brandy; but, in return, muſt endeavour to learn in ſome meaſure to underſtand the Dutch language. Thoſe who reſide among the planters learn it with great eaſe; but thoſe who live at a diſtance muſt embrace ſome other opportunity of learning it: and when the *Monturs* go to the Cape Town, they muſt always carry with them ſome men belonging to their horde, to whom various occupations, ſuch as cutting wood, &c. are aſſigned in the caſtle, that they may become familiar with the language by frequenting the company of the ſoldiers, and the inhabitants. When the Montur goes to the Cape the next year, he carries with him ſome more of his countrymen, and thoſe who came the preceding year return. Theſe Hottentots are perhaps the lazieſt people in the world; but in many parts of their country they are deſtitute of water. They leave the moſt fertile fields uncultivated, and only plant here and there a little Turkiſh corn. Their chief occupations are the rearing of cattle and hunting, but they purſue the latter only when compelled by neceſſity and the want of food. They pay little attention to fiſhing, though they have very good opportunities, eſpecially in winter, of catching fiſh without great exertion. There have been inſtances of their enduring hunger for ſeveral days rather than give

give themſelves the leaſt trouble to ſearch for food. The Company have frequently offered to ſupply them with nets, corn, and other articles, and to aſſign lands to them nearer the Cape, if they would only become a little more active and induſtrious—but without the leaſt effect; as they believe that in this caſe they ſhould be expoſed to more bodily labour. They prefer living in poverty in the midſt of the deſerts, in a ſtate little ſuperior to that of their cattle; and are ſo timid as to ſuffer themſelves to be driven ſometimes towards the South by the Caffres, and ſometimes towards the Eaſt by the Boſhmen. If they could only aſſume a little courage, they would ſoon make their enemies ſenſible of their power; but they have very little care for the future.

" The women are very active in the management of their domeſtic economy, but have a conſiderable degree of vanity, for they employ a great deal of their time in ornamenting their perſons. To a mother a child is a great treaſure, and the huſband dare never preſume to take it into his hands as long as it is at the breaſt, though in other reſpects he treats his wife as his ſlave. This right has been claimed by the mothers in conſequence of the cruelties formerly practiſed by the fathers; for, when the Portugueſe firſt viſited this country, many of the men ſold their children to them for brandy or tobacco; this incenſed the mothers, and therefore they aſſumed the above privilege. A child is ſuckled for four months, during which time the mother in the day carries it about on her back wrapped up in a ſheep's ſkin, but in the night places it in her boſom; after that period it is ſuffered to lie on a mat ſpread out on the graſs, and has the ſame food as the reſt of the family.

" I was much pleaſed with their treatment of the dead. They never bury them until every mean poſſible has been tried to recall them to life. Thoſe who die of chronic or lingering diſeaſes, they ſhake and beat on the ribs with their fiſts; if they exhibit no ſigns of life, they are interred the ſame evening, but not until ſimilar experiments have been tried to revive them. On young perſons who die ſuddenly other methods are tried: the ſoles of their feet are pricked with

with aſſagays *, pieces of red hot iron are applied to their noſe, and their bodies are well rubbed; but if no ſigns of life appear, they are committed to the earth next evening. The corpſe is immediately carried out from the hut, and watched by ſome of the relations, who do the ſame thing for twenty-eight days after it has been buried, in order to prevent it from being torn up by wild beaſts.

Our traveller reſumed his journey on the 25th, directing his courſe towards Fiſh River, and on the 29th entered Caffraria, where at firſt he had to aſcend ſteep mountains, but towards evening deſcended into the level country. After paſſing through ſeveral hordes of Caffres, by whom he was in general well received, he arrived at a kraal conſiſting of 127 huts, and containing 1400 inhabitants. As he was here treated with great friendſhip, he remained ſome weeks; and having heard from the natives, among whom he ſaw ſeveral European articles, that a ſhip had been ſtranded on the coaſt, at a place lying at the diſtance of two days journey, he reſolved to go thither, and prevailed on ſome of the Caffres to accompany him. They accordingly ſet out on the 27th of April, and next evening reached the remains of the veſſel, which were lying in a bay between Fiſh River and the river St. Lucia. Here they found ſeveral half-putrid bodies, moſt of them pierced with wounds, ſcattered about on the ſhore, and intermixed with caſks, cheſts, bales of cotton, and other articles, entirely ſpoilt by the ſea water and the ſun. What chiefly attracted the notice of the Caffres was the iron work belonging to the ſhip, of which they collected as much as they could. After burying the bodies †, and picking up various articles, among which was a pocket compaſs, they ſet out to return. The compaſs, which was not injured, fell

* The points of their aſſagays are not poiſoned except when they go out to attack their enemies.

† This ſhip had been loſt in conſequence of a mutiny which broke out among the crew, who firſt murdered their officers, and then ran the veſſel on ſhore. The crew then landed, and wandered about in the country, where they either fell a ſacrifice to the climate or were maſſacred by the Caffres. Of the whole ſhip's company four only eſcaped, and of theſe four two died of their wounds. The other two made their way to the Cape.

to

to the lot of our traveller, who conſidered it as an acquiſition of no ſmall importance.

On the 20th of May, Damberger took leave of theſe friendly Caffres, and, after encountering conſiderable dangers and difficulties between that period and the 11th of July, directed his courſe eaſtwards, with a view of proceeding to Egypt. Towards evening he diſcovered ſome huts at a diſtance, but was not able to reach them; he therefore lay down on the graſs with his head on his bundle to enjoy a little reſt, but was ſoon rouſed by four Caffres, who conducted him to their huts. Here he was robbed of his fowling-piece and bundle, but they were both afterwards reſtored. He was attacked alſo with a violent pain in his bowels; but on making known his complaint to one of the Caffres, he gave him ſome dried leaves to chew, which acted as a ſtrong emetic, and ſoon afforded him relief.

Among the next horde whom he viſited he was more unfortunate, being robbed both of his bill and his fowling-piece; the former he recovered, but the latter was broken to pieces by the Caffres in order that they might convert the iron part of it into points for their aſſagays.

Soon after leaving this horde he was obliged to paſs the night in the open air on the banks of a rivulet, where he made a large fire to frighten away the wild animals, but was unable to ſleep a moment on account of the multitude of ſnakes which ſwarmed around him; and towards morning he ſaw hundreds of large baboons. The latter had taken up their ſtation on all the neighbouring trees, and did not ſeem in the leaſt ſhy or afraid.

Of theſe people the author ſays, "This nation, as well as ſeveral more of the African tribes, are accuſed of indolence, but I am convinced that the induſtrious Europeans, if tranſported hither, would be equally inactive. The heat oppreſſes the labourer too much, and exhauſts the powers of the body; to this it may be added, that moſt of theſe tribes are deſtitute of the neceſſary implements, ſuch as ſpades, hoes, &c. and the ground is ſo hard that, before any other tool can be uſed, it muſt be cut up with a ſort of pickaxe. The cultivation of the few fields, which are ſown here with Turkiſh wheat, conſumes

conſumes a great deal of time and requires much labour. I obſerved that two ſtout active men could till in a day no more than ſix feet ſquare. The land, after being ſown, was covered, to the height of two inches, with ſand, in order that the exceſſive heat might not too ſoon dry up the moiſture, and to prevent the ground from cracking by the drought and the rain. They derive more advantage, without much labour, from rearing of cattle, and from hunting and fiſhing. This nation, indeed, have a ſtrong propenſity to ſtealing, but they never commit murder; and much leſs do they devour human fleſh. Several Europeans *, however, who have viſited theſe people, aſſert that they eat human fleſh with great avidity; but this is abſolutely falſe. If a traveller underſtand their language he has nothing to fear, eſpecially if badly clothed and in poſſeſſion of no articles of value. If they take any thing from him, he may reſt aſſured that they will give him ſomething of theirs in return if he aſks for it.

"The dead are interred at ſome diſtance from the kraal, and the relations kindle a large fire near the ſpot, which they maintain three days, to prevent the ſavage animals, that might be attracted by the ſmell, from coming to tear up the body. Few perſons die in the kraals, as the ſick are removed to huts ſituated at a diſtance; for they believe that every diſeaſe is infectious, and that it is their duty to guard againſt the contagion."

After a ſtay of three weeks among theſe people, whom our traveller calls the Jamatians, he continued his journey, and, croſſing the Makumbo river, entered the territories of the Muhotians. Here he was at firſt treated with great harſhneſs and ſeverity, but he ſtill found friends among the female ſex. While he remained with this tribe he was conducted to a place where he found the bodies of five white men covered with wounds, and apparently pierced with aſſagays: on their right arms he obſerved the figure of a crucifix, with the letters H, I, E, M, and the date 1779.

On the ſecond of October he arrived among another tribe called the Kamtarians, who inhabit a diſtrict ſituated on the river Tambo, and who live chiefly by hunting and the rear-

* Kolbe, Sparmann, Patterſon, &c.

ing of cattle. The men in general are of low ſtature, and have ſhort woolly hair. Theſe people were of a darker complexion than the other tribes he had quitted. They conſiſted of ſix or ſeven thouſand, of both ſexes, capable of bearing arms. The women were of larger ſize than the men, and exceedingly bold and courageous. The greater part of them had been carried away by force from other nations, or had been taken priſoners in war and retained by the men. Theſe people never rear their female children, but deſtroy them as ſoon as they are born. There are no prieſts among them, though there is reaſon to think that they are not deſtitute of religion. The oldeſt inhabitant in each village acts generally in the capacity of chief and judge. Polygamy is allowed among this tribe; and every woman not pregnant by her huſband is at liberty to cohabit with any other man whom ſhe chooſes, and if ſhe bring forth a boy to him, ſhe may become his wife.

Theſe people are exceedingly ſuperſtitious, and, when the leaſt misfortune befalls them, ſhut themſelves up in their huts, where they remain totally inactive for three or four days. When any one of their nation has been wounded or killed by wild beaſts while hunting, they ſay "he offended the great god;" but if ſuch an event take place in the night-time, and particularly by moon-light, they ſay, "he offended the little god," and on that account was puniſhed. If a woman die in childbed, which, however, is ſeldom the caſe, ſhe is buried in a particular ſpot, and her family muſt remain ſix moons, at ſome diſtance from the village, in huts ſet apart for that purpoſe, without having any intercourſe with the reſt of the tribe. If a woman bring forth a male child during cloudy weather, or when the moon does not ſhine, it is concluded that his father has offended the gods, and that the boy is not worthy of being a reſpectable member of ſociety. When he grows up, therefore, he is employed in the moſt menial offices, ſuch as cutting wood, guarding the cattle, and other ſervices of the like kind. Neither circumciſion, nor any ſimilar practice, is known in this country.

If a woman be delivered of a boy at a fortunate period, the father kills a goat, and gives an entertainment to the friends of

of his family. The dead are generally buried by their relations under large trees. During the time of the funeral a large fire is kindled, in which the effects of the deceased are burnt, and the ashes are thrown into the grave. The fire is kept burning till the next full moon. Whoever steals a married or unmarried female, may keep her as his wife; but, if he does not choose to do so, he may sell her, and in return generally receives a sheep, or a greater or less number of assagays, according to her beauty. These people are kind and hospitable to strangers.

Damberger left the Kamtarians on the 7th of October, and on the 30th, after passing through part of the kingdom of Biri, arrived in the territories of that of Mataman. The sovereign of this country rules with unlimited power, and is stiled *Sohaawoia*, that is, Chosen by the gods. The crown is hereditary to females as well as males. When one of the former succeeds, she may choose from her subjects a husband to participate with her in the government. The person so chosen, however, must be first examined by the oldest part of the community, in order that it may be known whether he possesses the talents necessary for the intended dignity. The king here acts in the capacity of high-priest, prophet, and tutor to the youth. His decisions are respected even when he judges wrong. He possesses the exclusive privilege of having a plurality of wives, and must install the inferior judges, priests, &c. He never takes the field against the enemy, but delegates the command of the army to another. The soldiers are brave, and exceedingly expert in managing their bows and arrows. They are said to amount to about 30,000. There are three large cities in the kingdom, the capital of which, called Seenhofa, lies at the distance of two days journey. This city, the residence of the king, our traveller resolved to visit; and, being furnished with a person to show him the way, soon reached the place. On his arrival he repaired to the *Mohwoia**, who gave him some corn and four milk, and conducted him into his hut, where he was

* This is the name given to the governors appointed in each town by the king, and who at the same time act as priests.

allowed to paſs the night. Next morning the Mohwoia carried him through a long ſtreet, bordered on each ſide with huts, to a kind of green, where he was introduced to the king, whom he found ſtanding amidſt a circle of his attendants, and who appeared to be about forty years of age. The day before he had taken four florins from his veſt*, in order that he might make an offer of them to the ſovereign. This he accordingly did, and requeſted his majeſty's protection and a ſupply of food. The prince, after looking ſome time at the money, ordered one of his wives to fetch ſome milk and meal, which being mixed together and ſtirred round, our traveller ſat down on the ground and made a hearty meal. He then accompanied the king into his hut, where a mat was brought for him to ſit upon. The king aſked him for ſome more money, and told him that in return he would make him a *kahſeeto* (ſervant). Damberger proteſted that he had none; but, as he was deſirous of becoming acquainted with the manners and cuſtoms of the country, he informed the king that he gratefully accepted his offer. Obſerving, however, ſome days after, that he was conſidered in the light of a common ſlave, he reſolved to make his eſcape the firſt opportunity he could find for that purpoſe.

On the 29th of November, having been obliged to accompany the king his maſter on a hunting excurſion, for the purpoſe of carrying a calabaſh filled with water, a leather bag containing millet, and a javelin, in paſſing through a wood interſperſed with ſmall hills he pretended to be ſuddenly taken ill with a pain in his bowels, and immediately ſat down on one of theſe eminences. The king, not ſuſpecting any deception, gave him permiſſion to remain till he ſhould call for him, and, taking the javelin from him, proceeded forwards; but as ſoon as his maſter was out of ſight, our traveller ſet off as faſt as he could, purſuing a northern direction with a view of reaching the mountains. In croſſing a river he ſtumbled againſt a ſtone and hurt his foot, by which means his progreſs was conſiderably retarded; and falling in ſoon after with ſome of

* Some time before this period the reſt of his clothes had become ſo ragged and filthy that he was obliged to burn them.

the

the natives, they compelled him to take upon his back a young wolf they had found, and to return with them to a village which he had left only a few hours before. Here he was detained eight days, during which he ſuffered conſiderably from the wound in his foot; but being at length ſuffered to depart, and proceeding towards the territories of the Seegerians, he arrived at a village called Mukoſa, ſituated on an eminence.

[To be continued.]

VI. *Experiments on the Solar and on the Terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame, or different.* By WILLIAM HERSCHEL, *LL.D. F.R.S.*

[Continued from Page 134.]

14th Experiment. Refraction of the Heat of a Chimney-Fire.

I PLACED Mr. Dollond's lens before the clear fire of a large grate *. Its diſtance from the bars of the grate was three feet; and in the ſecondary focus of it was placed the thermometer No. 1. No. 4 was ſtationed, by way of ſtandard, at 2 inches from the former, and at an equal diſtance from the fire. Before the thermometers was a ſlip of mahogany, which had three holes in it, $\frac{8}{10}$ of an inch in diameter each. Behind the centre of the firſt hole, $\frac{3}{8}$ of an inch from the back, was placed the thermometer No. 1; and between the ſecond and third hole, guarded from the direct rays of the fire by the partition, at the ſame diſtance from the back, was put No. 4. Things being thus arranged, the ſituation of the apparatus which carried the thermometers, and that where the lens was fixed, were marked. Then the thermometers, having been taken away to be cooled, were reſtored to their places again, and their progreſs marked as follows:

* See Plate VIII. fig. 3.

No.

	No. 1. Burning lens.	No. 4. Screened.	
0′	58	58	Here, in nine minutes, the rays coming from the fire, through the burning glaſs, gave $9\frac{3}{4}$ degrees of heat more to the thermometer No. 1, than No. 4, from change of temperature, had received behind the ſcreen. Now, to determine whether this was owing merely to a tranſmiſſion of heat through the glaſs, or to a condenſation of the rays, by the refraction of the burning lens, I took away the lens, as ſoon as the laſt obſervation of the thermometers was written down, and continued to take down their progreſs as follows:
$1\frac{1}{2}$	65	60	
3	68	61	
5	70	$61\frac{1}{2}$	
7	$71\frac{1}{4}$	$61\frac{3}{4}$	
9	$71\frac{1}{2}$	$61\frac{3}{4}$	

	No. 1.	No. 4.	
$9'\frac{1}{2}$	$71\frac{1}{2}$	$61\frac{1}{4}$	Here the direct rays of the fire, we ſee, could not keep up the thermometer No. 1; which loſt $2\frac{1}{4}$ degrees of heat, notwithſtanding the lens intercepted no longer any of them. I now reſtored the burning glaſs, and continued.
11	$70\frac{1}{2}$	$61\frac{3}{4}$	
12	$70\frac{1}{4}$	$61\frac{3}{4}$	
—	$69\frac{1}{2}$	$61\frac{3}{4}$	
$14\frac{1}{2}$	$69\frac{1}{4}$	$61\frac{3}{4}$	

$15'$	$69\frac{1}{4}$	$61\frac{3}{4}$	Here again, the lens acted as a condenſer of heat, and gave $1\frac{3}{4}$ degrees of it to the thermometer No. 1. I now once more took away the lens, and continued the experiment.
16	$69\frac{1}{2}$	$61\frac{3}{4}$	
17	70	$61\frac{3}{4}$	
20	$70\frac{3}{4}$	$61\frac{3}{4}$	
25	71	$61\frac{3}{4}$	

$25'\frac{1}{2}$	71	$61\frac{3}{4}$	This again confirms the ſame by a loſs of 3 degrees of heat. I reſtored the lens once more, and had as follows:
31	68	$61\frac{3}{4}$	

	No. 1. Burning Lens.	No. 4. Screened.	
$31'\frac{1}{2}$	68	$61\frac{3}{4}$	And here the thermometer received $1\frac{1}{2}$ degree of heat again; ſo that, in the courſe of 35 minutes, the thermometer No. 1 was alternately raiſed and depreſſed five times, by rays which came from the chimney fire, and were ſubject to laws of refraction, not ſenſibly different from thoſe which affect light.
35	$69\frac{1}{2}$	$61\frac{3}{4}$	

15th Experiment. Refraction of the Heat of red-hot Iron.

I cauſed a lump of iron to be forged into a cylinder of $2\frac{1}{2}$

inches diameter, and $2\frac{1}{2}$ inches long *. This, being made red-hot, was ſtuck upon an iron handle fixed on a ſtand ſo as to preſent one of its circular faces to a lens placed at 2.8 inches diſtance; its focus being 1.4 inch, and diameter 1.1. Before the lens, at ſome diſtance, was placed a ſcreen of wood, with a hole of an inch diameter in it, by way of limiting the object, that its image in the focus might not be larger than neceſſary. The ſcreen alſo ſerved to keep the heat from the thermometers. No. 2 was ſituated in the ſecondary focus of the lens; and No. 3 was placed within $\frac{3}{10}$ of an inch of it, and at the ſame diſtance from the lens as No. 2. By this arrangement, both thermometers were equally within the reach of tranſmitted heat; or, if there was any difference, it could only be in favour of No. 3, as being behind a part of the lens which, on account of its thinneſs, would ſtop leſs heat than the middle. Now, as the experiment gives a reſult which differs from what would have ariſen from the ſituation of the thermometers, on a ſuppoſition of tranſmitted heat, we can only aſcribe it to a condenſation of it by the refraction of the lens; and, in this caſe, the thermometer No. 3, by its ſituation, muſt have been partly within the reach of the heat-image formed in the focus. During the experiment, the thermometers were alternately ſcreened two minutes from the effects of the lens, and expoſed to it for the ſame length of time, and the reſult was as follows:

		No. 2. In the Focus.	No. 3. Near the Focus.	
Screened	0′	56	56	Here, in the firſt and ſecond minutes, No. 2 gained two degrees of heat more than No. 3. In the third and fourth, it loſt one
Open	2	62	60	
Screened	4	59	58	
Open	6	61	59	
Screened	8	$58\frac{1}{4}$	$57\frac{3}{4}$	
Open	10	$59\frac{1}{2}$	$58\frac{1}{4}$	

more than No. 3. In the fifth and ſixth, it gained one more. In the ſeventh and eighth, it loſt $1\frac{1}{2}$ more; and in the ninth and tenth, it gained $\frac{3}{4}$ more than the other thermometer. This plainly indicates its being acted upon by refracted heat. Leſt there ſhould remain a doubt upon the ſubject, I now re-

* See Plate VIII. fig. 1.

moved

moved the lens, and, putting a plain glaſs in the room of it, I repeated the experiment with all the reſt of the apparatus in its former ſituation.

Screened	0′	57$\frac{1}{4}$	56$\frac{3}{4}$	Here we find that both thermometers received heat and parted with it always in equal quantities, which confirms the experiment that has been given.
Open	2	62$\frac{1}{4}$	61$\frac{3}{4}$	
Screened	4	60$\frac{1}{2}$	60	
Open	6	61	60$\frac{1}{2}$	
Screened	8	60	59$\frac{1}{2}$	
Open	10	60$\frac{1}{2}$	60$\frac{1}{2}$	

And thus it is evident that there are rays iſſuing from red-hot iron, which are ſubject to the laws of refraction, nearly equal to thoſe which affect light; and that theſe rays are inveſted with a power of cauſing heat in bodies.

16*th Experiment. Refraction of Fire-heat, by an Inſtrument reſembling a Teleſcope.*

It occurred to me that I might uſe a concave mirror to condenſe the heat of the fire in the grate of my chimney, and, reflecting it ſideways by a plain mirror, I might afterwards bring it to a ſecondary focus by a double convex lens; and that, by this conſtruction, I ſhould have an inſtrument much like a Newtonian teleſcope *. The thermometer would figuratively become the obſerver of heat, by being applied to the place where, in the real teleſcope of the ſame conſtruction, the eye is ſituated to receive light. Having put together the different parts, in ſuch a way as I ſuppoſed would anſwer the end, I tried the effect by a candle, in order to aſcertain the proper diſtance of the object-mirror from the bars of the chimney-grate. The front of the apparatus was guarded by an iron plate, with a thick lining of wood; and the two thermometers which I uſed, were parted from the mirrors and lens by a partition, which ſcreened them from the heat that was to be admitted through a proper opening in the front plate, to come at the object-mirror. In the partition was likewiſe an opening, of a ſufficient diameter to permit the rays to come from the eye-glaſs to their focus, on the ball of the thermometer No. 1; while No. 4 was placed

* See Plate VIII. fig. 2.

by

by the ſide of it, at leſs than half an inch diſtance. In the experiment, the object-mirror was alternately covered by a piece of paſteboard, and opened again. The thermometers were read off every minute; but, to ſhorten my account, I only give the laſt minute of every change.

		No. 1. In the Focus.	No. 4. Near the Focus.	Here, in the
The mirror covered	0′	$77\frac{1}{2}$	$77\frac{1}{2}$	firſt eight
The mirror open	8	84	76	minutes,
Covered -	16	$86\frac{1}{2}$	$79\frac{1}{2}$	the ther-
Open -	21	$89\frac{3}{4}$	81	mometer
Covered -	27	$89\frac{3}{4}$	$82\frac{1}{4}$	expoſed
Open -	37	$91\frac{1}{2}$	$83\frac{1}{2}$	to the ef-
Covered -	47	84	77	fects of

the fire-inſtrument, gained 2 degrees of heat more than the other. In the next 8 minutes, the mirror being covered, it gained 1 degree leſs than the other. The mirror being now opened again, it gained, in five minutes, $2\frac{3}{4}$ degrees more than the other. When covered ſix minutes, it gained $1\frac{1}{4}$ degree leſs than No. 4. In the next ten minutes, when open, it gained $\frac{1}{2}$ degree more; and, in the laſt ten minutes, when the fire began to fail, and the mirror was again covered, it loſt one degree more than the other thermometer. All which can only be accounted for by the heat which came to the thermometer through the fire-inſtrument; and, as this experiment confirms what has been ſaid before of the refraction of culinary heat, ſo it alſo adds to what has already been proved of its reflection. For, in this fire-inſtrument, the rays which occaſion heat could undergo no leſs than two reflections and two refractions.

17*th Experiment. Refraction of the inviſible Rays of Solar Heat.*

I covered one-half of Mr. Dollond's burning lens with paſteboard, and threw the priſmatic ſpectrum upon that cover*; then, keeping the laſt viſible red colour one-tenth of an inch from the margin of the paſteboard, I let the inviſible rays beyond the ſpectrum fall on the lens. In the

* See Plate I. fig. 2.

focus of the red rays, or a very little beyond it, I had placed the ball of the thermometer No. 1; and, as near to it as convenient, the ſmall one No. 2. Now, that the inviſible ſolar rays which occaſion heat were accurately refracted to a focus, may be ſeen by the following account of the thermometers:

	No. 1. In the Focus.	No. 2. Near the Focus.	
0′	57	57	Here, in one minute, theſe rays gave 45 degrees of heat to the thermometer No. 1, which received them in the
1	102	57	

focus, while the other, No. 2, ſuffered no change.

It is remarkable that, notwithſtanding I kept the red colour of the ſpectrum $\frac{1}{10}$ of an inch upon the paſteboard, a little of that colour might ſtill be ſeen on the ball of the thermometer. This occaſioned a ſurmiſe that, poſſibly, the inviſible rays of the ſun might become viſible if they were properly condenſed; I therefore put this to the trial, as follows:

18*th Experiment. Trial to render the inviſible Rays of the Sun viſible by Condenſation.*

Leaving the arrangement of my apparatus as in the laſt experiment, I withdrew the lens, till the laſt viſible red colour was two-tenths of an inch from the margin of the ſemicircular paſteboard cover; then, taking the thermometers, I had as follows:

	No. 2.	No. 3.	
0′	57	57	Here there was no longer the leaſt tinge of any colour, or veſtige of light, to be ſeen on the ball of
1	78	57	

the thermometer; ſo that, in one minute, it received 21 degrees of heat, from rays that neither were viſible before, nor could be rendered ſo by condenſation.

To account for the colour which may be ſeen in the focus, when the laſt viſible red colour is leſs than two-tenths of an inch from the margin of the paſteboard which intercepts the priſmatic ſpectrum, we may ſuppoſe that the imperfect refraction of a burning lens, which, from its great diameter, cannot bring rays to a geometrical focus, will bring ſome ſcattered ones to it, which ought not to come there. We may alſo admit that the termination of a priſmatic ſpectrum cannot

cannot be accurately aſcertained by looking at it in a room not ſufficiently dark to make very faint tinges of colour viſible. And to this muſt be added that the incipient red rays muſt actually be ſcattered over a conſiderable ſpace, near the confines of the ſpectrum, on account of the breadth of the priſm, the whole of which cannot bring its rays of any one colour properly together; nor can it ſeparate the inviſible rays entirely from the viſible ones. For, as the red rays will be but faintly ſcattered in the beginning of the viſible ſpectrum; ſo, on the other hand, will the inviſible rays, ſeparated by the parts of the priſm that come next in ſucceſſion, be mixed with the former red ones. Sir Iſaac Newton has taken notice of ſome imperfect tinges or hazineſs on each ſide of the priſmatic ſpectrum, and mentions that he did not take them into his meaſures *.

19*th Experiment. Refraction of inviſible Culinary Heat.*

There are ſome difficulties in this experiment; but they ariſe not ſo much from the nature of this kind of heat, as from our method of obtaining it in a detached ſtate. A red-hot lump of iron, when cooled ſo far as to be no longer viſible, has but a feeble ſtock of heat remaining, and loſes it very faſt. A contrivance to renew and keep this heat might certainly be made, and I ſhould, indeed, have attempted to carry ſome method or other of this kind into execution, had not the following trials appeared to me ſufficiently concluſive to render it unneceſſary. Admitting, as has been proved in the 15th experiment, that the alternate riſing and falling of a thermometer placed in the focus of a lens, when the ball of it is ſucceſſively expoſed to, or ſcreened from, its effects. is owing to the refraction of the lens, and cannot be aſcribed to a mere alternate tranſmiſſion and ſtoppage of heat, I proceeded as follows † :—My lens, 1·4 focus, and 1·1 diameter, being placed 2·8 inches from the face of the heated cylinder of iron, the thermometer No. 2, in its focus, was alternately guarded by a ſmall paſteboard ſcreen put before it, and expoſed to the effects of condenſed heat by removing it.

* Newton's Optics, p. 23, l. 11. † See Plate VIII. fig. 1.

		No. 2.	
Screened	0′	55	Very red-hot.
Open	2	$63\frac{1}{2}$	Red-hot.
Screened	4	58	Still red-hot.
Open	6	$60\frac{1}{2}$	Still red.
Screened	8	$57\frac{1}{2}$	A little red.
Open	10	$59\frac{1}{2}$	Doubtful.
Screened	12	$57\frac{1}{2}$	Not visible in my
Open	14	$58\frac{1}{2}$	room darkened.
Screened	16	$57\frac{1}{2}$	
Open	18	$58\frac{1}{2}$	
Screened	20	$57\frac{1}{2}$	
Open	22	58	
Screened	24	$57\frac{1}{2}$	
Open	26	58	
Screened	28	$57\frac{1}{2}$	

Now, the beginning of this experiment being exactly like that of the 15th, with the thermometer No. 3 left out, the arguments that have before proved the refraction of heat in one state, will now hold good for the whole. For here we have a regular alternate rising and falling of the thermometer, from a bright red heat of the cylinder, down to its weakest state of black heat; where the effect of the rays, condensed by the lens, exceeded but half a degree the loss of those that were stopped by it.

20*th Experiment. Confirmation of the* 19*th.*

In order to have some additional proof, besides the uniform and uninterrupted operation of the lens in the foregoing experiment, I repeated the same with an assistant thermometer, No. 3, placed first of all at $\frac{1}{4}$ of an inch from No. 2, and more towards the lens, but so as to be out of the converging pencil of its rays, and also to allow room for the little screen between the two thermometers, that No. 3 might not be covered by it.

		No. 2. In the Focus.	No. 3. Advanced sideways. Always open.
Screened	0′	$62\frac{1}{2}$	63
Open	1	$63\frac{1}{4}$	64
Screened	2	$62\frac{7}{8}$	64
Open	3	64	$64\frac{1}{2}$
Screened	4	$63\frac{3}{4}$	$64\frac{1}{2}$
Open	5	$64\frac{7}{8}$	$64\frac{1}{2}$
Screened	6	$64\frac{1}{2}$	$64\frac{1}{2}$
Open	7	$64\frac{3}{4}$	64
Screened	8	$64\frac{1}{2}$	64

Here No. 3, being out of the reach of refraction, gradually acquired its maximum of heat, in consequence of an uniform exposure to the influence of the hot cylinder; after which it began to decline. No. 2, on the

the contrary, came to its maximum by alternate great elevations and ſmall depreſſions; and afterwards loſt its heat by great depreſſions and ſmall elevations. After the firſt eight minutes, I changed the place of the aſſiſtant thermometer, by putting it into a ſtill more deciſive ſituation; for it was now placed by the ſide of that in the focus, ſo as to participate of the alternate ſcreening, and alſo to receive a ſmall ſhare of one ſide of the inviſible heat-image, which, though unſeen, we know muſt be formed in the focus of the lens. Here, if our reaſoning be right, the aſſiſtant thermometer ſhould be affected by alternate riſings and fallings; but they ſhould not be ſo conſiderable as thoſe of the lens.

		No. 2. In the Focus.	No. 3. In the Edge of it.	Here the changes of the thermometer No. 2 were $-\frac{3}{4}+\frac{1}{2}$ $-1\frac{1}{4}+\frac{3}{4}-1+1$; and thoſe of No. 3 were $-\frac{1}{4}+\frac{1}{4}-\frac{3}{4}+\frac{1}{4}-\frac{1}{2}+\frac{3}{4}$.
Both open	8′	$64\frac{1}{2}$	64	
Both open	9	$63\frac{3}{4}$	$63\frac{3}{4}$	
Open	11	$64\frac{1}{2}$	64	
Screened	$12\frac{1}{2}$	63	$63\frac{1}{4}$	
Open	14	$63\frac{3}{4}$	$63\frac{1}{2}$	
Screened	16	$62\frac{3}{4}$	63	
Open	18	$63\frac{3}{4}$	$63\frac{3}{4}$	

All which ſo clearly confirm the effect of the refraction of the lens, that it muſt now be evident that there are rays iſſuing from hot iron, which, though in a ſtate of total inviſibility, have a power of occaſioning heat, and obey certain laws of refraction, very nearly the ſame with thoſe that affect light.

As we have now traced the rays which occaſion heat, both ſolar and terreſtrial, through all the varieties that were mentioned in the beginning of this paper, and have ſhown that, in every ſtate, they are ſubject to the laws of reflection and of refraction, it will be eaſy to perceive that I have made good a proof of the three firſt of my propoſitions. For the ſame experiments which have convinced us that, according to our ſecond and third articles, heat is both reflexible and refrangible, eſtabliſh alſo its radiant nature, and thus equally prove the firſt of them.

End of the Firſt Part.

Slough, near Windſor,
April 26, 1800.

VII. *Account*

VII. *Account of ſome intereſting Experiments, performed at the London Philoſophical Society, reſpecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a Number of Gems and other refractory Subſtances ſubmitted to its Action; with a Deſcription of the Apparatus employed.*

[Continued from Page 29.]

Wedgewood's Pyrometer Pieces.

XVII. AFTER the experiments already deſcribed, it was propoſed by ſeveral of the members that ſome attempt ſhould be made to determine the degree of heat which had been excited; and, with a view to this end, a fragment of one of Mr. Wedgewood's pyrometer pieces was ſubjected to the blaſt: in a few ſeconds, however, it became perfectly fuſed. It does not appear, therefore, that we are at preſent in poſſeſſion of any better inſtrument for appreciating the intenſity of heat thus produced than the gems themſelves, which, as ſome of them are more refractory than others, and many of them more ſo than moſt other ſubſtances, may, from the changes which they undergo, ſerve to afford a rough method of eſtimating the comparative degree of temperature.

Spinel.

XVIII. A ruby-ſpinel, which weighed $\frac{24}{64}$ths of a carat, was, like the preceding ſtones, expoſed in an excavation made in a piece of charcoal ignited by means of a common blow-pipe, to a ſtream of oxygen gas from the gaſometer. At the end of $2' 3''$ it had loſt neither weight nor colour, but had aſſumed the appearance of a rough garnet, having apparently undergone a very ſlight and ſuperficial fuſion, juſt ſufficient to injure its poliſh.

Jargoons.

XIX. Theſe ſtones are colourleſs, and harder than rock-cryſtal, but leſs ſo than the ruby. They are uſually found in diamond-mines, and, when well cut and ſet, play nearly as well as roſe diamonds. That they are however eſſentially

different from the diamond, needs no other proof than their incombuſtibility.

One which weighed $\frac{14}{64}$ths of a carat, expoſed to the heat for 2′, was found to have loſt no weight; whereas a diamond of the ſame ſize would in that time have been entirely diſſipated, and reſolved into carbonic acid gas. The ſtone loſt its tranſparency, and aſſumed the appearance of a piece of white enamel: it had evidently begun to fuſe, but ſome traces of its form, and even of its facets, are ſtill diſcernible.

XX. Another jargoon, weighing $\frac{4}{64}$ths of a carat, was expoſed to the heat for the ſame length of time. It broke into three pieces while under the blaſt, but now preſents the ſame appearances as the former. The ſurfaces of both are more opake and white than the interior.

Vermilions.

XXI. A vermilion which weighed $\frac{8}{64}$ths of a carat, treated for 2′, loſt no weight, but became fuſed into a poliſhed opake globule, which is nearly black, with a tinge of browniſh-green, like that of very dark bottle glaſs.

XXII. Fourteen vermilions, weighing $\frac{48}{64}$ths of a carat, expoſed for 2′ 30″, were completely fuſed into a poliſhed opake globule, very like the former, but not quite ſo dark. It is alſo not ſo round or poliſhed, having been preſſed while ſoft. A ſmall face being afterwards ſubjected to the lapidary's wheel, the ſtone appeared as hard as at firſt, but not ſuſceptible of ſo high a poliſh.

Garnet.

XXIII. One which weighed $1\frac{8}{64}$ths of a carat, expoſed to the blaſt for 1′ 40′, broke in two pieces, both of which became fuſed. The largeſt preſents an appearance very like that of the vermilion No. XXI.; the other is of a dark lead colour inclining to a bronzy hue and chatoyant, ſomewhat like peacock-lead ore. Both were preſſed while ſoft; the latter ſo much ſo, that it at preſent retains nothing of a globular form.

Emeralds.

XXIV. One, of the weight of $\frac{10}{64}$ths of a carat, was fuſed in 2 into a globule of an opake white colour without any loſs

loſs of weight. It exactly reſembles a pearl from a common oyſter, or "dead pearl," as jewellers term it.

XXV. Another emerald, weighing $\frac{20}{64}$ths of a carat, being treated for the ſame length of time, preſents the ſame appearance as the former, except that about one-half of the globule ſtill retains ſome traces of its original colour, and is now of a greeniſh gray.

Chryſolite.

XXVI. A chryſolite, weighing $\frac{16}{64}$ths of a carat, being treated during 2′ loſt no weight, but became fuſed into a rough opake greeniſh black globule.

Jacynth.

XXVII. One jacynth, weighing $\frac{44}{64}$ths of a carat, treated for 1′ 14″ loſt no weight, but became fuſed into a globule much reſembling one of dark coloured bottle-glaſs, but nearly opake.

Opal.

XXVIII. An opal, weighing $\frac{14}{64}$ths of a carat, treated for 44″ melted into a poliſhed globule of a greeniſh white almoſt tranſparent, but full of ſmall bubbles. It loſt no weight.

Cryſtals.

XXIX. A cut white cryſtal, weighing $\frac{22}{64}$ths of a carat, treated 1′ 44″ melted into a perfectly tranſparent globule like flint glaſs, but full of cracks and bubbles. Its weight is the ſame as before.

XXX. Two fragments of rock-cryſtal, weighing together $1\frac{32}{64}$ carat, expoſed to the heat during 5′ 19″ melted together, but flew to pieces on cooling, and the whole is now in ſeveral fragments; ſome of it even in powder. It reſembles a very pure white ſalt.

Platina.

XXXI. Sixteen carats of platina in grains, in the crude ſtate, were perfectly fuſed in a few ſeconds. A ſecond and a third quantity were likewiſe fuſed; but the circumſtance moſt remarkable was, that the globules thus formed were found to be pure and malleable. On examining them attentively it was found that the iron, which is always preſent

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in crude platina, and which, by adhering to it with great obſtinacy, makes it difficult to obtain that metal in its pure ſtate, was completely oxydated, and adhering ſo looſely to the ſurface of the globules that it ſeparated itſelf in ſcales when they were ſtruck with a hammer. The globules were then extended under the hammer without breaking.

From this experiment it appeared probable that means might be deviſed to fuſe large quantities of crude platina, and at the ſame time to obtain the metal pure and malleable; an object ſo deſirable, that the Society reſolved at leaſt to make the attempt.

XXXII. A table furnace was conſtructed in ſuch a manner, that the crucible containing the platina (eight ounces) could be brought to a ſtrong heat, by means of charcoal all round it and a pair of double bellows, before allowing the ſtream of oxygen gas to be introduced into it. The platina was put into the centre of the crucible, with charcoal below and all round, in ſuch a manner that the crucible was filled with the charcoal and the metal. The cover was luted on, and a tube, made of burnt clay, which paſſed from a hole near the bottom of the crucible to the outſide of the furnace, was firmly luted into the crucible, which was ſupported in the middle of the furnace. The clay tube was connected, by means of another tube, with the large gaſometer, and that again with a ſeries of caſks filled with oxygen gas.

After exciting, by means of the double bellows, the fire round the outſide of the crucible, till it was thought the crucible and its contents muſt have attained the utmoſt degree of heat that could in this way be obtained, the communication between the gaſometer and the interior of the crucible was opened, and a ſtream of oxygen gas forced in through the clay tube among the contents of the crucible.

It will be obſerved from what has been ſtated, that the intention was not to excite the fire in the body of the furnace by means of a ſtream of oxygen. That appeared to be an unneceſſary waſte. All that was aimed at was, by exciting a ſudden and rapid combuſtion of the charcoal lodged in the crucible along with the platina, to reduce the latter; and it was thought that the previous ignition of the crucible and its contents, by the help of the furnace, would facilitate this.

In a few ſeconds after opening the communication with the gaſometer, ſuch a vivid flame iſſued through the hole in the cover of the furnace, that it was thought unneceſſary to carry the proceſs further, as the gas appeared evidently to be acting on the fuel in the furnace, inſtead of having its action confined principally, as was intended, to the contents of the crucible.

After ſome time, the furnace was examined. More than a half of the clay tube which ſerved to convey the gas through the furnace into the crucible was found gone, and the remaining portion of the tube poliſhed highly on the ſurface, the clay having been running in a ſtate of extreme fuſion. The ſide of the crucible next the tube was poliſhed in the ſame manner, and the hole in which the tube had been luted was found greatly enlarged. The charcoal in the crucible had not been half conſumed, and the platina was found fuſed at the bottom. A few globules were found interſperſed among the charcoal, having been arreſted in their deſcent by ſtopping the proceſs before they had time to reach the chief maſs at the bottom; but it ſeemed plain that, had the blaſt of oxygen gas been continued for a minute or two longer, the crucible would have been completely run down.

The platina was then examined, but the reſult was not what the Society had promiſed themſelves. Though it had been completely fuſed, the proceſs was found to have been of too ſhort a duration to oxydate all the iron contained in it. The large button broke under the hammer.

From this it appears that, though platina, in ſmall quantities (Exper. XXXI.) may be reduced and brought to a ſtate of purity by oxydating the iron by means of a ſtream of oxygen gas, it will require a conſiderable degree of addreſs to be able to apply this on a ſcale of any conſiderable extent. If even rock-cryſtal, and thoſe gems which, till theſe experiments, have been held infuſible, are yet found fuſible by the powerful heat excited by the agents employed in them, how are materials to be obtained ſufficiently refractory to maintain the neceſſary arrangements in a furnace during ſuch a proceſs?

VIII. *An*

VIII. *An Examination of* ST. PIERRE's *Hypotheſis reſpecting the Cauſe of the Tides, which, in oppoſition to the received Theory, attributes them to ſuppoſed periodical Effuſions of the Polar Ices. By* SAMUEL WOODS, *Eſq. Read before the Aſkeſian Society November* 5, 1799.

[Concluded from Page 147.]

I NOW proceed to ſtate the three remaining proofs adduced by St. Pierre in corroboration of the demonſtration I have juſt noticed; but, as I conceive myſelf to have fully diſproved the geometrical evidence, I ſhall not trouble you with an attempt to invalidate theſe ſubſidiary confirmations.

The ſecond proof (ſays he) is atmoſpherical. It is well known, that, in proportion as you aſcend a mountain, the mercury in the barometer ſubſides: now the mercury ſinks in the barometer in proportion as you advance northward. The weight of one line of mercury at Paris is equivalent to an elevation of 10 fathom and 5 feet, whereas in Sweden it is equivalent to 10 fathom 1 foot 6 inches only; and of courſe the ground of Sweden muſt be higher. From a ſeries of obſervations made by captain Cook in the ſouthern hemiſphere in 1773 to 1775, we perceive the mercury ſcarcely ever riſes higher than 29 inches beyond the 60th degree of ſouth latitude, and mounted almoſt always to 30 inches and even higher in the vicinity of the torrid zone; which proves that the barometer falls as you recede from the line, and that both poles are elongated.

The third proof is nautical, ariſing from the annual deſcent of the ices toward the line, impelled by currents proceeding alternately from each pole during their reſpective ſummers, immenſe mountains of ice being frequently ſeen by navigators in low latitudes.

The fourth proof is aſtronomical. Childrey (an Engliſh author of note) ſuppoſes, as I do, that the earth at the poles is covered with ice to ſuch a height as to render its figure ſenſibly oval. Kepler ſays that the eclipſe of the moon on the 26th September 1624, like the one obſerved by Tycho Brahè in 1588, which was total, and very nearly central,

differed widely from the calculation: for, not only the duration of total darkneſs was extremely ſhort, but the reſt of the duration, previous and poſterior to the total obſcuration, was ſtill ſhorter, as if the figure of the earth was elliptical, having the ſmalleſt diameter under the equator, and the greater from pole to pole.

Navigators in the north have always ſeen the elevation of the ſun above the horizon greater the nearer they approach the poles. It is impoſſible to aſcribe theſe optical effects to atmoſpherical refraction.

Barents, on the 24th of January, in Nova Zembla, ſaw the ſun 15 days ſooner than he expected, which would give a refraction of $2\frac{1}{2}^{\circ}$; a thing impoſſible, and the circumſtance can be aſcribed to no other cauſe than his real elevation.

St. Pierre cuts the difficulty ariſing from the different vibrations of the pendulum, by obſerving that they are liable to a thouſand errors.

The elongation of the poles being thus demonſtrated, the current of the ſeas and tides follows as a natural and neceſſary conſequence.

Let us now conſider the extent of the polar ices, and the powers capable of effecting their ſolution.

The polar ices in the winter proper to each hemiſphere are from ſix to ſeven thouſand leagues in circumference; but in their ſummer, from two to three thouſand.

The ices and ſnows form in our hemiſphere, in January, a cupola, the arch of which extends more than two thouſand leagues over the two continents, with a thickneſs of ſome lines in Spain, ſome inches in France, ſeveral feet in Germany, many fathoms in Ruſſia, and beyond the 60° of north lat. of ſome hundred feet. Some ice iſlands were ſeen by Ellis from fifteen to eighteen hundred feet above the level of the ſea, and they probably go on increaſing to the pole to a height indeterminable. Hence the enormous aggregation of water, fixed by the cold of winter in our hemiſphere, above the level of the ocean, is clearly perceptible; and to the periodical fuſion of theſe vaſt maſſes the general movement of the ſeas and tides is juſtly aſcribable. The ices at the ſouth pole exceed in quantity thoſe at the north; and two ſuch

bodies

bodies of ices, alternately accumulated and diſſolved, at the two poles, muſt occaſion a very perceptible augmentation of its waters at their return to it by the action of the ſun, and a great diminution by their reduction to ice when the ſun retires. It has been calculated that the earth and ſea covered with ice, may be equalled to 1-10th of the whole ocean, and the height of the polar ices is at leaſt 600 feet; a maſs which in melting muſt add 1-10th, that is 60 feet, to the level of the ocean.

Nature has diſtributed ſandy zones to aſſiſt, at the proper ſeaſon, in accelerating the fuſion of the polar ices. The winds in ſummer convey the igneous particles with which theſe zones are filled towards the poles, where they aſſiſt the ſun's action on the ices.

The moon alſo diſſolves ice by the humidity of the atmoſphere. When the moon ſhines in winter nights in all her luſtre it freezes very ſharply, becauſe the north wind checks the evaporating influence of the moon: but if the wind is ſtilled ever ſo little, you ſee the heavens covered with vapours which exhale from the earth, and you feel the atmoſphere ſoftened.

Nature having determined to indemnify the poles for the ſun's abſence, makes the moon paſs toward the pole, which the ſun abandons: ſhe cryſtalliſes, and reduces into brilliant ſnows, the waters which cover it; ſhe renders its atmoſphere more refractive, that the ſun's preſence may be detained longer in it, and reſtored ſooner to it: and hence alſo there is reaſon to conclude ſhe has drawn out the poles of the earth in order to beſtow on them a longer participation of the ſun's influence. We may judge from analogy the general effect of the tides: A ſource diſcharging itſelf into a baſon produces at the ſides of that baſon a backward motion or counter current, which carries ſtraws and other floating ſubſtances up towards the ſource.

Charlevoix (Hiſt. of New France) tells us that, though the wind was contrary, he ſailed at the rate of eight leagues a day up lake Michigan, againſt its general current, by the aſſiſtance of its lateral counter-currents.

M. de Crevecœur aſſures us, that in ſailing up the Ohio, along its banks he made 422 miles in 14 days, or ten leagues a-day,

a-day, by means of the counter-currents, which have always a velocity proportional to that of the principal current.

The particular effects observed in lakes and rivers communicating with icy mountains, illustrate the nature of the polar effusions. A kind of flux and reflux in the lake of Geneva, during summer and towards the evening, is observable, occasioned by the melting of the snows, which fall into it after noon in greater quantities than at other seasons of the day. The intermittence of certain fountains is ascribable to the same cause. The frequent and rapid fluxes (ten or twelve times a day) of the Euripus, the strait separating Bœotia from Eubœa, arise from the same source.

The currents of the ocean are reducible to two general ones: one, during our summer, from the north pole, in a south direction; the other, during our winter, proceeding northward from the south pole.

Dampier lays it down as a principle, founded on many experiments, that currents are scarcely ever felt but out at sea, and tides upon the coasts.

The polar effusions, which are the tides of the north and east to those who dwell in the vicinity of the pole, or in bays communicating with it, take their general course to the middle of the channel of the Atlantic ocean, attracted toward the line by the diminution of the waters, which the sun is incessantly evaporating. They produce by their general current two contrary currents or collateral whirlpools similar to those produced by rivers on their banks, and the tides may be considered as vortices of the general current of the Atlantic ocean.

The general current, which flows from our pole in summer with so much rapidity, and which is so violent towards its source, crosses the equinoctial line, its flux not being stemmed by the effusions of the south pole, at that season consolidated into ice; it extends beyond the Cape of Good Hope, and being directed east, by the position of Africa and Asia, forces the Indian ocean into the same direction, and may be considered as the prime mover of the western monsoon, which takes place in the Indian seas in April, and ends in September.

The general current, issuing during our winter from the

ſouth pole, reſtores the Indian ocean to its natural motion weſt; croſſes, in its turn, the equinoctial line, penetrates into our Atlantic ocean, directs its motion north by the poſition of America, and produces various changes in our tides. All the bays, creeks, and mediterraneans of ſouthern Aſia, ſuch as the gulphs of Siam and Bengal, the Perſian gulph, the Red ſea, &c. are directed relatively to theſe currents north and ſouth ſo as not to be ſtemmed by them; as all the bays and mediterraneans of Europe, as the Baltic, the Engliſh channel, the bay of Biſcay, the Mediterranean ſea, Baffin's bay, Hudſon's bay, the gulph of Mexico, and many others, are directed relatively to theſe currents eaſt and weſt; or, to ſpeak with more preciſion, the axes of all the openings of the land in the old and new world are perpendicular to the axes of theſe general currents, ſo that their mouth only is croſſed by them, and their depth is not expoſed to the impulſions of the general movements of the ocean.

That theſe currents are not the offspring of my own imagination, but actually ſuch as I have deſcribed them, will appear from various teſtimonies. Froger ſays that in Brazil the currents follow the ſun, running ſouthward when he is ſouth, and northward when he is north. In the ſummer of the ſouthern hemiſphere, the tides ſet in northward (*Schouten, Jan.* 1661), but in winter run ſouthward and come from the north (*Fraſer, May* 1712). C. Columbus ſet ſail from the Canaries the beginning of September, and ſteered to the weſt; he found, during the firſt days of his voyage, that the currents carried him to the north-eaſt; when he had advanced 200 or 300 leagues from land, he perceived their direction was ſouthward: finally, as he approached the Lucayo iſlands, he again found the current ſetting in north.

The nautical obſervations of Cook demonſtrate that the currents of the Atlantic ocean are alternate and half-yearly like thoſe of the Indian ocean. The beans called Oxeyes, which grow only in the Weſt Indies, are every year thrown up on the coaſt of Ireland, 1200 leagues diſtant. Seeds and turtles are brought to the Hebrides from the Weſt Indies and America; and the maſt of the Tilbury man of war, burnt at Jamaica, was found on theſe coaſts: the current

which

which wafts these along proceeds in a north direction, and proves that the Atlantic current comes from the south, and sets in north during our winter. The currents of the north annually convey, in summer, toward the south, long banks of floating ices of very considerable depth and elevation, which run aground as far south as the banks of Newfoundland.

Rennefort (June 20, 1666), near the Azores (in lat. 40° to 45°), saw the broken masts, sailyards, &c. wrecked in the engagement which lasted four days between the English and Dutch, from June 11 to 15: this naval combat took place 12 miles to the north-west of Ostend, about 51° north. The currents from the north had therefore wafted them in nine days 11° south, besides a considerable progress westward.

The general current issuing from the south pole divides into two branches; one, setting in towards the Atlantic ocean, penetrates even to its northern extremity. This part, straitened by the prominent parts of Africa and America, forms on the coast two counter currents, which proceed in opposite directions. One of these currents runs east, along the coast of Guinea, to the 4th degree of south latitude; the other takes its departure from Cape St. Augustin, proceeding south-west, along Brasil, to Maire's Straits. In the middle of the Atlantic ocean, beyond the strait formed by the two continents, this general branch pushes on north, and advances to the north extremities of Europe and America, bringing us twice every day along our coasts the tides of the south, which are the half daily effusions of the two sides of the south pole. The other branch takes a direction south of Cape Horn, rushes into the South sea, produces the monsoon in the Indian ocean, and, having made the tour of the globe, unites itself by the Cape of Good Hope to the general current which enters the Atlantic ocean.

In our summer, commencing toward the end of March, when the sun retires from the southern hemisphere, and proceeds to warm the north, the effusions of the south pole are stayed, those of our pole begin to flow, and the currents of the ocean change throughout every latitude. The general current of our seas divides also into two branches; the first deriving its source from Waigats, Hudson's Bay, &c. flows with

with the rapidity of a ſluice, deſcends through the Atlantic ocean, croſſes the line, and, finding itſelf confined at the ſame Strait of Guinea and Braſil, forms two lateral counter currents ſetting in north: theſe counter currents produce, on the coaſts of Europe, the tides which appear to come from the ſouth. The general current advances ſouth, arrives about the month of April at the Cape of Good Hope, and renders the paſſage round this cape ſo difficult to veſſels returning from India at this ſeaſon; about the middle of May it reaches the coaſts of India, produces the weſt monſoon, and, having encompaſſed the globe, proceeds to Cape Horn, re-aſcends the coaſts of Braſil, and creates a current terminating at Cape St. Auguſtin.

The other general branch, which receives much leſs of the icy effuſions, iſſues between the continents of Aſia and America, and deſcends to the South ſea, where it is re-united to the firſt branch. The ocean accordingly flows twice a year round the globe in oppoſite ſpiral directions, taking its departure alternately from each pole, and deſcribes on the earth the ſame courſe which the ſun does in the heavens.

The courſe of our tides towards the north in winter is not an effect of the lateral counter currents of the Atlantic ocean, but of the general current of the ſouth pole, which runs north. In this direction almoſt throughout it paſſes from a wider ſpace into a narrower, and carries before it at once the whole maſs of the waters of the Atlantic ocean, without permitting a ſingle column to eſcape either to the right or left. However, if it meet a cape or ſtrait to oppoſe its courſe, it would form there a lateral current, as at Cape St. Auguſtin, and in Africa about 10° N. lat.; for in the ſummer of the ſouth pole the currents and tides return ſouth on the American, and eaſt on the African ſide, the whole length of the Gulph of Guinea, in contradiction to all the laws of the lunar ſyſtem.

From theſe polar effuſions the principal phænomena of the tides may be explained. It will be evident, for example, why thoſe of the evening ſhould be ſtronger in ſummer than thoſe of the morning; becauſe the ſun acts more powerfully by day than by night on the ices of the pole on the ſame meri-

dian as ourſelves: and alſo why our morning tides in winter riſe higher than thoſe of the evening, and why the order of our tides changes every ſix months; becauſe, the ſun being alternately towards both poles, the effect of the tides muſt be oppoſite, like the cauſes which produce them. At the ſolſtices the tides are lower than at any other ſeaſon of the year, and thoſe likewiſe are the ſeaſons when there is moſt ice on the two poles, and conſequently leaſt water in the ſea: the reaſon is obvious, the winter ſolſtice is with us the ſeaſon of the greateſt cold; of cōurſe there is the greateſt poſſible accumulation of ice on our pole and hemiſphere. At the ſouth pole it is indeed the ſummer ſolſtice; but little ice is then melted, becauſe the action of the greateſt heat is not felt there as with us, till the earth has an acquired heat ſuperadded to the ſun's action, which takes place the ſix weeks following the ſummer ſolſtice.

At the equinoxes, on the contrary, we have the higheſt tides; and theſe are preciſely the ſeaſons when there is leaſt ice at the two poles, and of courſe the greateſt quantity of water in the ocean. At our autumnal equinox in September, the greateſt part of the ices of the north pole is melted, and thoſe of the ſouth pole begin to diſſolve. The tides in March riſe higher than thoſe in September, becauſe it is the end of ſummer to the ſouth pole, which contains much more ice than ours, and conſequently ſends a greater maſs of water to the ocean.

I ſhall ſay nothing (he proceeds) of the intermittence of the polar effuſions, which produce on our coaſt two fluxes and two refluxes nearly in the ſame time that the ſun, making the circuit of the globe, alternately heats two continents and two oceans, that is, in the ſpace of 24 hours, during which his influence twice acts and is twice ſuſpended; nor ſhall I ſpeak of the retardation, which is nearly $\frac{3}{4}$ of an hour every day, and which ſeems regulated by the different diameters of the polar cupola of ice, whoſe extremities, melted by the ſun, diminiſh and retire from us every day, and whoſe effuſions muſt conſequently require more time to reach the line, and to return from the line to us. Nor ſhall I dwell on the other relations theſe polar periods have to the phaſes of the moon, eſpecially

eſpecially when ſhe is at full; for her rays poſſeſs an evaporating heat, as the late experiments at Rome and Paris fully demonſtrate *; much leſs ſhall I involve myſelf in a diſcuſſion of the tides of the ſouth pole, which in ſummer in the open ſea come in vaſt ſurges from the ſouth and ſouth-weſt. There are two tides every day; becauſe the ſun warms by turns, every 24 hours, the eaſt and weſt ſide of the pole in fuſion. Preciſely the ſame effect takes place in lakes ſituated in the vicinity of icy mountains, which have a flux and reflux in the day-time only. But it cannot be doubted that, if the ſun warmed, during the night, the other ſide of theſe mountains, they would produce another flux and reflux; and conſequently two tides in 24 hours, like the ocean.

We are not to imagine that every tide is a polar effuſion of the particular day on which it happens, but an effect of the ſeries of polar effuſions; ſo that the tide which takes place on our coaſts to-day, is perhaps part of that which took place ſix weeks ago. But here, too, muſt we admire the harmony of nature: the evening and morning tides take place on our coaſts as if they iſſued that very day from the higher and lower part of our hemiſphere; and the tides of ſummer are preciſely oppoſite to the tides of winter, as are the tides from whence they flow; our evening tides in ſummer, and our morning tides in winter, being greateſt.

If the tides are ſtronger after the full moon, it is becauſe that luminary increaſes by her heat the polar effuſions, and conſequently the quantity of water in the ocean.

Let us now explain why the tides of the South ſea do not reſemble thoſe of the Atlantic ocean. The irregular effuſions of the poles, not being narrowed in the ſouthern hemiſphere, as in ours, produce on the ſhores of the Indian ocean and South ſea expanſions vague and intermitting. The ſouth pole has not, like the north pole, a double continent, which ſeparates into two the divergent effuſions daily produced by the ſun: it has no channel in paſſing through which its effluxes ſhould be retarded: its effuſions accordingly flow directly into the vaſt ſouthern ocean, forming on the half of that pole a ſeries of divergent emanations which perform the

* I do not know where any account of theſe ſtrange experiments can be found.—W.

tour, of it in 24 hours, like the rays of the sun. When a bundle of these effusions falls upon an island, it produces there a tide of twelve hours, *i. e.* of the same duration with that which the sun employs in heating the icy cupola through which the meridian of that island passes; such are the tides of the islands of Otaheite, Massafuero, New Holland, New Britain, &c.: each of these tides lasts as long as the course of the sun above the horizon, and is regular like his course.

In the nothern part of the South sea the two continents approach: they pour therefore by turns, in summer, into the channel which separates them, the two semi-diurnal effusions of their pole, and there they collect by turns, in winter, those of the south pole, which produces two tides a day as in the Atlantic ocean. But as this channel about the 55° of N. lat. ceases to exist by the sudden divergence of the continents of Asia and America, those places only situated in the point of divergence of the northern parts of these two continents experience two tides a day. Such are the Sandwich Islands. Where such places are more exposed to the current of the one continent than the other, its two semi-diurnal tides are unequal, as at the entrance of Nootka Sound: but when it is completely out of the influence of the one, and entirely under that of the other, it receives only one tide of twelve hours every day, as at Kamtschatka. Thus, two harbours may be situated in the same sea under the same parallel, and one of them have two tides, and the duration of these tides, whether double or single, double equal or double unequal, regular or retarded, is always 12 hours every 24 hours, *i. e.* precisely the time the sun employs in heating that half of the polar cupola from whence they flow; which cannot possibly be referred to the unequal course of the sun between the tropics, and much less to that of the moon, which is frequently but a few hours above the horizon of such harbours. All islands are in the midst of currents: on looking therefore at the south pole with a bird's-eye view, we should see a succession of archipelagos dispersed in a spiral line all the way to the northern hemisphere, which indicates the current of the sea, just as the projection of the two continents on the side of the north pole indicates the current of the Atlantic. Thus,

the

the courſe of the ſeas from one pole to the other is in a ſpiral line round the globe, like the courſe of the ſun from one tropic to the other: admitting therefore the alternate fuſion of the polar ices, all the phænomena of the tides and currents of the ocean may be explained with the greateſt facility.

I have then eſtabliſhed by facts ſimple, clear, and numerous, the diſagreement of the tides in moſt ſeas with the moon's action on the equator, and their perfect coincidence with the ſun's action on the polar ices.

I have no doubt various objections may be urged againſt this haſty explanation of the courſe of the tides, &c. But theſe phyſical cauſes preſent themſelves with a higher degree of probability, ſimplicity, and conformity to the general progreſs of nature, than the aſtronomical cauſes by which it is attempted to explain them.—Thus far St. Pierre.

I heſitate whether I ought not to apologiſe for occupying ſo much of your time and attention in the detail of a theory which may be deemed unworthy of ſerious notice; yet I flatter myſelf it will afford an opening to curious and intereſting diſcuſſion. St. Pierre complains that the prejudices of mankind are ſo ſtrong in favour of received opinions, that he cannot obtain a hearing.

To the beſt of my judgment I have offered a fair and candid expoſition of a hypotheſis which he has dreſſed up with ſome eloquence and much declamation, and uſhered into the world with a ſolemn and impoſing air of confidence and aſſurance, tolerably well calculated to confound the ignorance and candour of his readers. I am not conſcious of having omitted any material fact or argument which tends to the ſupport and elucidation of his theory; I have neglected much abſurd reaſoning, yet not without retaining ſome curious ſpecimens. I did once intend to have entered into a general examination of his principles and reaſoning; to have ſhown the fallacy of the former, the inconcluſiveneſs and inconſiſtency of the latter; but I ſhall now be ſatisfied with offering a few facts and obſervations extracted from the 2d and third voyage of Captain Cook, which appear to me deciſive of the queſtion.

Captain

Captain Cook, who ſpent three ſummers as near as the ice would permit his approach towards the ſouth pole, found on December 14, 1772, and from that date to the beginning of January 1773, in latitude from 55° to 64° ſouth, a vaſt compact body of ice which prevented his further progreſs. The thermometer varied from 30° to 35°. Being immerſed 100 fathom deep for about 20 minutes, it came up 34°; and on the 13th of January 1774, on a repetition of this experiment, the open air being 36°, the ſurface of the ſea 33½°, the thermometer came up 32°. They found water generally freeze at 33°. "We certainly had no thaw, (ſays he,) the mercury keeping uſually below the freezing point. Being near an iſland of ice (December 24, 1772) 50 feet high and 400 fathom in circuit, I ſent the maſter in the jolly boat to ſee if any water ran from it. He ſoon returned with an account there was not one drop, or any other appearances of thaw." And in the ſummer of 1774—75 his experience was nearly ſimilar. On the 13th of February 1775, the thermometer ſtood at 29°. In his 3d voyage to the north-weſt coaſt of America, on the 17th of Auguſt 1778, in lat. 70° 44', they were ſtopped by a field of ice 10 or 12 feet high, as compact as a wall; "further north it appeared much higher; here and there we ſaw upon it pools of water; we tried, but found no current. July 7, 1779, lat. 69°; ſtopped by a large field of ice, preſenting a great extent of ſolid and compact ſurface not in the ſmalleſt degree thawed: the thermometer ſtood at 31°."

"As far as our experience went, the ſea is clearer of ice in Auguſt than in July, and perhaps it may be ſtill freer in a part of September. We tried the currents, and found them never to exceed a mile an hour; we found the month of July infinitely colder than Auguſt; the thermometer in July was once 28°, and very commonly 30°; whereas it was ſeldom as low as the freezing point in Auguſt."

"I am of opinion (ſays Captain Cook) that the ſun contributes very little towards reducing theſe vaſt maſſes of ice; for, although that luminary is a conſiderable time above the horizon, it ſeldom ſhines out more than a few hours at a time, and often is not ſeen for ſeveral days in ſucceſſion. It is the wind, or rather the waves raiſed by the wind, that reduces

the bulk of these enormous masses, by grinding one piece against another, and by undermining and washing away those parts that lie exposed to the surge; and more ice may be destroyed in one stormy season than is formed in several winters, and its accumulation thus prevented."

This evidence clearly proves that the sun's influence at the poles, so far from being equal to produce a constant and uniform effect, creating an impulse extending its effect to the remotest parts of our globe, and a daily elevation of several feet to the waters of the ocean, is not sufficient in the hottest period of summer to diffuse a sensible thaw; and thus we are convinced that a few plain and simple facts are of much greater avail than a multitude of fanciful conjectures.

NEW PUBLICATION.

A Manual of a Course of Chemistry; or, a Series of Experiments and Illustrations necessary to form a complete Course of that Science. By J. B. Bouillon Lagrange, *Professor in the Central Schools of Paris, &c.* Translated from the French, with 17 Plates. 2 Vols. 18 Shillings. Cuthel, and Vernor and Hood, 1800.

We have perused this work with much pleasure. It is comprehensive, though concise; and the manner in which the author treats his subject is well calculated to give to those who wish to study chemistry, not only a knowledge of the theory, but also, which ought to be the chief aim of every work of this kind, of the processes and manipulations as applicable to the arts and the common purposes of life.

The apparatus employed in modern chemistry is described with considerable accuracy and clearness, and illustrated with appropriate engravings; but in the original, the connection and relation of all the parts, though very correctly given *in outline*, are not sufficiently apparent and obvious, especially to tyros. The English publishers have done justice to the work, and even a service to the science of chemistry, by having their plates properly shaded and highly finished. It is enough of

of them to ſay, that they are from the graver of LOWRY, and equal in merit and execution to thoſe given in the Philoſophical Magazine.

The tranſlator, who has not given his name, has performed his taſk in a manner creditable to himſelf. He has ſhown himſelf ſomething more than a mere literary drudge: he has corrected ſeveral overſights of the author, or rather, perhaps, inaccuracies of the French printer, which, though often only a ſingle letter in the name of a compound body, were of importance. The terminations in *ates*, and *ites*, and *ats*, ſo uſeful in the modern nomenclature, have this inconvenience—they are ſo nearly ſimilar, that printers, not chemiſts, will often ſubſtitute the one for the other. We ſpeak this from experience, and we may be allowed to take this opportunity of recommending to chemical authors always to reviſe after the printer before their ſheets are put to preſs.

A ſhort appendix has been added by the tranſlator, containing ſome uſeful notes and notices of new facts diſcovered ſince the original work was publiſhed; and alſo ſeveral tables of French weights, &c.

We ſhall here give a few extracts, which, at the ſame time that they ſerve as a ſpecimen of the work and of the tranſlation, will furniſh information or amuſement to ſome of our readers.

Fluoric Acid.

For our knowledge of the fluoric acid we are indebted to Scheele.

This appellation was given to it becauſe it is extracted from a kind of earthy neutral ſalt, known under the name of *ſparry fluor*, *phoſphoric mineral*, *fluate of lime*.

As the fluoric acid diſſolves glaſs, it is neceſſary, when you wiſh to have it pure, to employ for that operation veſſels of a metal on which neither it nor the ſulphuric acid exerciſes any action: lead, among the known metals, is that which is beſt fitted for this purpoſe.

There are two methods of preparing this acid: 1ſt, With a metallic apparatus: 2d, With a glaſs apparatus.

1ſt, To obtain the fluoric acid alone, and free from every combination, put one part of the fluate of lime, reduced to powder,

powder, into a leaden retort; pour over it three parts of ſulphuric acid, and adapt to the retort a leaden receiver half full of water. The operation is performed with a balneum mariæ; and for this purpoſe you put the retort into a copper or iron veſſel containing water, or into a ſalt-bath; you then expoſe the apparatus to a gentle heat, and the fluoric acid, in proportion as it is diſengaged, will be abſorbed by the water in the receiver.

For this purpoſe, inſtead of a receiver, you adapt to the oriſice of the retort a bent tube of lead, the extremity of which is introduced under a mercurial pneumatic apparatus.

2d, When this acid is made by means of a glaſs apparatus, you employ a retort, having adapted to it a tube inſerted in a bottle containing diſtilled water.

As this acid has the property of diſſolving glaſs, it ſeizes on the ſilex, which appears under the form of white flakes.

Care muſt be taken to employ large tubes, eſpecially when you operate with glaſs, as for want of a ſufficient paſſage the gaſeous acid is compreſſed in the retort, and its action on the glaſs is augmented, ſo that the retort will be ſoon corroded through.

The ſilex depoſits itſelf in the water, becauſe the latter has more affinity for the acid than the acid has for the ſilex.

If you preſerve ſome of this gas under a glaſs bell, it will diſſolve the ſilex of the glaſs.

If you plunge into it an extinguiſhed taper, an incruſtation will be formed on the wick, becauſe the water which iſſues from it diſſolves the ſurrounding acid charged with ſilex, and the ſiliceous earth is precipitated upon it from this ſolution.

This gas is heavier than atmoſpheric air; it extinguiſhes a lighted taper, kills animals, reddens blue vegetable colours, and has a penetrating ſmell, which approaches near to that of muriatic acid gas.

It corrodes the ſkin: it undergoes no alteration from light.

In contact with the air it emits white fumes. If you expoſe to the vapour of this gas in an earthen veſſel animals, bits of ſponge a little moiſtened, charcoal, &c. the moiſture

they contain will diſſolve the acid, and the ſilex will be precipitated upon them.

If this experiment be made in a metal veſſel, the ſame incruſtation will not take place.

It thence reſults that the earthy ſubſtance which is precipitated by the contact of the fluoric acid gas and the water, is nothing elſe than a portion of the glaſs, which is attacked and actually diſſolved by the aëriform acid.

It often happens alſo, that when this gas is made to paſs into water, the ſilex is precipitated in a quartzy pellicle. Each bubble of the acid which touches the water is immediately enveloped in ſilex, and leaves on its way as it aſcends to the ſurface of the water, traces in the form of tubes, which Prieſtley calls *organ-pipes*, and which decreaſe to a point upwards, becauſe the bubble decreaſes in proportion as the water diſſolves it, and the ſilex is thus carried off.

The ſilex depoſited in the veſſels is ſoon after re-diſſolved by the exceſs of acid, in proportion as the water is ſaturated; for the water, being at firſt little ſaturated with the acid, has not ſufficient ſtrength to hold the ſilex in ſolution.

Bergman obtained fluate of ſilex in a cryſtallized form.

When the fluoric acid is made in earthen veſſels, you will have ſilex depoſited, and then re-diſſolved by the re-action of the acid: this is a real fluate of ſilex inſtead of pure fluoric acid.

Alkalies may be employed for detecting the preſence of the ſilex. The taſte of this acid, diſſolved in water, is like that of the ſulphuric acid diluted with water, or of vinegar.

If a ſolution of fluoric acid in water be expoſed to heat, part of the acid is volatilized; but the laſt moleculæ adhere ſo ſtrongly, that the water and the reſt of the acid are volatilized, if the heat be increaſed.

This acid is preſerved in bottles covered on the inſide with wax diſſolved in oil, or in veſſels of lead or of platina.

Guyton employed this property to engrave labels on bottles, and in particular on thoſe deſtined to hold acids, the labels of which when made of paper are always burnt.

The elements of this acid are ſtill entirely unknown.

[To be continued.]

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ROYAL SOCIETY OF LONDON.

THE ſittings of the 17th November, the 11th and 18th December, were occupied in reading an intereſting paper on the mechaniſm of the eye, by Dr. Young.

St. Andrew's day falling on a Sunday, the anniverſary meeting was held on the 1ſt of December. The auditor's report on the ſtate of the finances of the Society was read. The names of the new members, thoſe deceaſed, and thoſe withdrawn, were declared; and the names of the officers juſt elected were announced. The Right Hon. Preſident then addreſſed the Society, congratulating them on the progreſs of ſcience in the laſt year, and concluded by informing them that Sir Godfrey Copley's medal was adjudged to Mr. H. Howard for his paper on fulminating mercury. Sir Joſeph Banks then informed the Society that Mr. Howard had alſo diſcovered a new fulminating ſilver, and is now engaged on a ſubject that promiſes to prove highly intereſting to meteorologiſts and mineralogiſts, namely, the analyſis of ſtones that have fallen from the clouds.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES, AND COMMERCE.

This Society, which has rendered much ſervice to the arts by its numerous premiums, that its efforts may, if poſſible, be rendered ſtill more generally uſeful, has juſt circulated the following notice:

Adelphi, Dec. 8. 1800.

Premiums by the Society for the Encouragement of Arts, Manufactures, and Commerce.

The time of propoſing and publiſhing the premiums to be given by this Society for the enſuing year now approaching, the patrons of the arts, and all ingenious men, are hereby invited to ſuggeſt new objects, to which the Society may extend its liberality.

Communications are requeſted to be made in writing, and addreſſed to the Society's ſecretary, at their houſe in the Adelphi, on or before the 1ſt of February 1801.

N. B. The 18th volume of the Tranſactions of this Society is now in the preſs, and will ſpeedily be publiſhed.

By order, CHARLES TAYLOR, Sec.

BOARD OF AGRICULTURE.

This Board has juſt publiſhed the following liſt of premiums:

This Board having been required, by a committee of the Houſe of Lords, 'to examine into, and report to their lordſhips, the beſt means of converting certain portions of graſs land into tillage, without exhauſting the ſoil, and of returning the ſame to graſs, after a certain period, in an improved ſtate, or at leaſt without injury;' and being deſirous that their information, on a ſubject of ſo great importance, ſhould be complete, adapted to every ſort of ſoil, and founded on the moſt ample experience, have come to the reſolution of offering the following premiums for that purpoſe, *viz.*

To the perſon who ſhall produce, on or before the firſt day of February 1801, the beſt and moſt ſatisfactory eſſay on the ſubject before mentioned, diſtinguiſhing, reſpectively, what part of the plan recommended, or of the details given, is the reſult of actual experiment, accurate obſervation, or well authenticated information, 200*l.* For the ſecond beſt, 100*l.* For the third beſt, 60*l.* For the fourth beſt, 40*l.*

And to ſuch perſons who may communicate information, which, though uſeful, may be conſidered of leſs importance, ſmaller rewards, proportioned to the opinion of the Board.

It is required that each eſſay ſhall fully detail the courſe of crops, regard being had to the varieties of ſoil, and the time propoſed for continuing the land under tillage.

Alſo, to explain the caſes in which it may be eligible to drain land previous to tillage.

In what caſes paring and burning are advantageous, with directions thereon, regard being had to the ſubſequent cropping.

The depth to which graſs lands ſhould, at firſt breaking up, be ploughed.

Whether the crops intended for cattle and ſheep are to be fed on the land, and by which kind of ſtock, or carted off.

To ſtate the crop with which the graſs ſeeds in each caſe ought to be ſown, when the land ſhall be again laid down:—The ſorts and quantities of graſs ſeeds for each kind of ſoil, and whether to be provided by landlord or tenant:—Whether it be beſt to mow or feed the graſs in the firſt year after laying down; to detail the management in each caſe:—The manuring which may be thought neceſſary:—The principle on which an increaſe of rent ought to be eſtimated, where permiſſion may be given to break up old paſture now under leaſe.

The Board requires that theſe objects ſhould be particularly attended to, with relation to the leading qualities of land, *viz.* —Clay, in all its diſtinctions; and ſoils too ſtrong or wet for turnips:—Loam, in all its diſtinctions, fit for turnips:—Sand, including warrens and heaths, as well as rich ſands:—Chalk land and downs:—Peat, including moory, ſedgy, rough bottoms, and fens.

It is hoped that no perſon will be deterred from communicating his knowledge to the Board on account of his experience being confined to one of theſe ſoils only.

The Board reſerves the power of withholding any premium, in the caſe of no eſſay being deemed ſufficiently important to merit it.

The eſſays which ſhall obtain any premium, or other reward, to remain the property of the Board.

Each eſſay to be ſent (ſealed) without any name, but with a mark or motto; and accompanied by a ſealed letter with the ſame mark or motto, containing the name and addreſs of the

the author; and this letter will not be opened, unleſs one of the prizes, or ſome other reward, ſhall be adjudged to him.

All communications to be addreſſed to Lord Carrington, preſident, Sackville-ſtreet.

FRENCH NATIONAL INSTITUTE.

The following account of the labours of the Phyſical and Mathematical Claſs during the laſt three months of the year 8 was read by C. Delambre.

C. Meiſſier has given a curious compariſon of the ſummer of the year 1800 and that of the year 1792. It reſults from his obſervations, that if the duration of the heat was nearly the ſame at each of theſe periods, it is certain that in the year 1792 the thermometer kept, pretty conſtantly, two degrees and a half higher than in the year 1800; but what renders the laſt ſummer more remarkable is, the extraordinary lowneſs of the water of the Seine.

In the year 1792, the water, for two months and a half, kept, pretty conſtantly, at about an inch above zero of the ſcale on the bridge de la Tournelle. During four days only, it ſunk ſo low as zero. On the 11th of Auguſt laſt it was ſtill lower by 4·9 inches. On the 20th of Auguſt it was even 7·65 inches below zero. It appears, then, that in the year 1800, the depreſſion of the water of the Seine was ſomething more than ſix inches greater than it was in 1792.

The year 1719 was remarkable alſo for a long drought and depreſſion of the river. During the whole of that year there fell no more than nine inches four lines of water at the obſervatory of Paris, which is not the half of the uſual quantity. The depreſſion of the water appeared then ſo extraordinary, that, to preſerve the remembrance of it, the ſcale of the bridge La Tournelle was conſtructed. This precaution, however, did not prevent us from having 325 millimetres of uncertainty in regard to the level of the Seine at that period. According to Parcieux, the depreſſion of the water in 1719 was indicated by the zero of the ſcale. According to Buache, it correſponded to the firſt foot of that ſcale. Meiſſier is inclined to adopt the latter indication. But however this may be, it is certain that in the year 1800 the river fell lower

than at any period ever remembered. According as we follow the one or the other of the two authors above mentioned, the difference will be 16 or 48.

C. Meiſſier read alſo a note on the eclipſe of the moon which took place on the firſt of October laſt. The clouds prevented him from obſerving the commencement of it. Towards the middle the moon appeared at certain intervals; and he took advantage of theſe moments to meaſure twelve times the part eclipſed. At 11 h. 21′ the eclipſe ſeemed to be at an end: it was more certainly ſo at 11 h. 0′ 40″. According to the obſervations of C. Delambre, the end took place about 11 h. 0′ 30″; which agrees pretty well with what might have been expected.

C. Prony read a memoir containing a deſcription and the analytical theory of a new inſtrument, proper for meaſuring the length of a pendulum that ſwings ſeconds. It is well known, that the uſual method requires the niceſt attention, not only in regard to the form of the body made to oſcillate, but alſo that the oſcillations may not be prolonged beyond two hours at moſt, and that the reſults deduced from them may be always ſubordinate to the regularity of the time-piece which ſerves for compariſon; and in the laſt place, the neceſſity of fitting up, and taking to pieces the apparatus, may leave ſome doubts reſpecting the identity of the experiments made at different times, and in different places. All theſe inconveniences C. Prony propoſes to remedy. His new inſtrument ſuperſedes the neceſſity of paying attention to the form of the oſcillating body; the maſs of it is too conſiderable for the oſcillations to continue during the whole time that elapſes between two conſecutive paſſages of a ſtar over the ſame vertical, ſo that the time-piece of compariſon is of no further uſe than to count with more facility the oſcillations of the pendulum uſed for the experiment.

The theory of C. Prony appears to be clear and ſatisfactory, and he ſoon intends to ſubject his new pendulum to a trial. The ingenious method by which Borda found means to correct or obviate the inconveniences of the common method are not yet forgotten. His determination of the length of the pendulum received the approbation of all learned

learned men, aſſembled at Paris, for fixing the new meaſures. If it be difficult to attain to greater preciſion, it will at leaſt be intereſting to ſee how far the reſults obtained are confirmed by a method abſolutely different.

C. Flaugergues read ſeveral memoirs, containing obſervations on various ſubjects; as, on the phoſphoreſcence of earth worms; on the effects of thunder; on halos, or luminous rings around the ſun; experiments tending to prove that the ſhadows of opake bodies, expoſed in the open fields to the light of the ſun, when the ſky is ſerene, and projected on a white ſurface, are always blue, or, more correctly, of the colour of the heavens: and, in the laſt place, experiments from which it reſults, that the waves, produced at the ſurface of the water by percuſſion, do not produce a movement of tranſlation in the parts of the fluid, as has been believed, according to Newton, but only a continued depreſſion of intumeſcence, which is afterwards propagated circularly.

C. Briſſon has publiſhed Phyſical Principles of Chemiſtry, deſtined as a ſupplement to his Principles of Natural Philoſophy. The author compoſed this work chiefly for the uſe of the ſtudents in the central ſchools. It contains, in a clear and methodical order, an account of all the ſubſtances which fall within the province of chemiſtry, an analyſis of them, their ſpecific gravities and other moſt remakable properties, with a deſcription of the principal kinds of apparatus employed for chemical experiments*.

C. Lacroix has publiſhed his Treatiſe of Differences and Series, forming a continuation of his Treatiſe on the Differential and Integral Calculus. This important work comprehends in one ſyſtem the methods ſcattered throughout different academical collections, and in the works of the greateſt geometricians of modern times. The ſame author has publiſhed a ſecond edition of his Treatiſe of Trigonometry, and the application of Algebra to Geometry.

C. Arbogaſt has juſt publiſhed a large work, entitled The Calculus of Derivations. The author gives the name of derivations to quantities deduced from each other by an uniform

* We are happy to learn that a tranſlation of this very uſeful work is now in the preſs, and will ſoon be publiſhed.

proceſs

proceſs, which has a great reſemblance to that uſed for finding the differentials of all the orders.

This proceſs he applies to different polynomous quantities; he teaches the method of finding the derivations in their greateſt ſimplicity, and of forming them almoſt without any other trouble than that of writing them. Each term of the reſolution may be obtained directly, and without being obliged to paſs through the intermediate ones.

This new calculus may be applied to ſimple and double recurring ſeries, and to the general recurrence of ſeries. It will ſerve often alſo to give more generality to known theorems, more vigour and brevity to demonſtrations; in a word, it is of the greateſt utility in the differential and integral calculus, which indeed forms only a peculiar caſe of the calculus of derivations.

The foreign members of the commiſſion of weights and meaſures continue their information reſpecting the intereſt with which their different governments receive the models of the metre and chiliogramme. M. Bugge, director of the obſervatory and of the board of longitude at Copenhagen, informs us that theſe two models have been committed to his care. C. Tralles, the deputy from Helvetia, and the miniſter of arts and ſciences for that republic, gives reaſon to hope that the metric ſyſtem will be adopted in that country. All theſe letters have been printed. We muſt not here omit a circumſtance which adds to our hopes in regard to Helvetia; which is, that, by a fortunate chance, there exiſts a very ſimple and remarkable relation between the metre and the Zurich foot, one of the moſt uſual meaſures in Swiſſerland. This foot is exactly equal to three decimetres, or at leaſt it does not exceed it but by two centi-millimetres, that is to ſay, by a little leſs than the hundredth part of a line—a quantity that may be neglected in commerce, and which is often inappreciable even in the niceſt operations of the phyſical ſciences.

MISCELLANEOUS ARTICLES.

NATURAL HISTORY.

C. Guerſant, profeſſor of natural hiſtory at Rouen, tranſmitted lately to C. Cuvier, for the purpoſe of examination, a quantity of bones found in the rocks in the environs of Honfleur, by the late Abbé Bachelet, and which belonged to the cabinet of the central ſchool of Rouen. C. Cuvier has diſcovered among theſe bones thoſe of a kind of crocodile, hitherto unknown, and very different from the foſſil animal of Maeſtricht, which ſome conſider as a crocodile alſo. The jaw-bones of this crocodile of Honfleur reſemble in their prolongation thoſe of the cavial, only the teeth are not ſo equal, and the ſutures of the bones are differently figured. The moſt ſtriking difference is in the vertebræ; thoſe of all the known crocodiles have the anterior face of their body concave, and the poſterior convex; in that of Honfleur it is preciſely the contrary. The apophyſes of theſe vertebræ are alſo more complex than in the ordinary crocodiles.

This animal appears to be about 18 feet in length; its bones are petrified, and ſtrike fire with ſteel. Their hollow parts are filled with pyrites. They were found in a marley kind of ſtone, very hard, of a grayiſh colour, and from which they could not be diſengaged without difficulty. Beſides the bones of this crocodile, C. Cuvier has found others which ſeem to ariſe from ſmall animals of the cetaceous kind.

MEDICINE.

The following account of a very ſingular diſeaſe which attacks men and animals in the province of Chiacas, in the government of Potoſi, is extracted from the journal of Lima: "The malady, which Dr. Como Rueno calls *furioſa locura*, is one of the moſt remarkable facts in the hiſtory of this province. It is chiefly in the village of Tutari that it attacks men and animals, but among the latter thoſe (ſuch as the ox, horſe, ſheep, &c.) which have been brought from Europe; for the vigogne, the guanaco, and other quadrupeds of the country, are not expoſed to it. No force can reſtrain

a perſon attacked by this diſeaſe, during the firſt fits of phrenſy. Being totally a ſtranger to ſentiments of ſhame, he eſcapes from bed, and runs with violence to the mountains. He flies from precipice to precipice, and at laſt throws himſelf from the firſt ſteep rock he finds in his way. In general the unfortunate wretch is daſhed to pieces; but if, by ſome uncommon chance, the fall does not prove mortal, he ſoon recovers his reaſon and health, and has nothing more to fear from a return of this fatal malady. I ſhall not attempt, ſays the author, to inquire whether the mineral effluvia which riſe from the boſom of the earth in a country expoſed to volcanic convulſions, may not have as great a ſhare in this phænomenon as the conſtitution of the inhabitants; but what is certain is, that it often makes its appearance in the province. This fact, adds he, has ſo much analogy with what we read in Ovid's Metamorphoſes reſpecting the leap of Leucade, that the one would ſeem to have ſerved as the original of the other. Who knows, ſays he, whether the antient fable may not have originated from ſome malady ſimilar to the above?

A ſurgeon of Madrid has been able to diſſolve camphor in water by means of the carbonic acid. This camphorated ſolution, injected into the urethra of perſons afflicted with the ſtone, allays the pain almoſt inſtantaneouſly.

CHEMISTRY.

Mr. Hahneman, of Altona, has diſcovered a new fixed alkali, which he calls *pneum*, becauſe, when heated to redneſs, its volume is extended to twenty times its uſual ſize. It cryſtalliſes in large priſmatic hexaedral cryſtals terminated by two inclined faces, one of which appears to be triedral, and the other pentaedral. Theſe cryſtals neither run *per deliquium* nor effloreſce; but when pulveriſed they diſſolve at 300° of Fahrenheit in half their weight of water, and melt almoſt in their water of cryſtalliſation. At 65° of the ſame thermometer, 140 parts require for their ſolution about 500 parts of water. By cold they ſeparate from the water. They do not diſſolve in alcohol. This alkali produces but little effervеſcence with concentrated acids. With vitriolic acid it forms

forms a falt not foluble in alcohol, and with difficulty in water. The neutral falts formed with this alkali, and the nitrous, muriatic, and phofphoric acids, and thofe in particular formed with the acetous acid, diffolve with eafe not only in water but in alcohol. Its muriate cryftallifes in the form of feathers: its phofphate has a bitter taffe. All thefe falts, except thofe formed by the phofphoric acid, part with their acids by heat, and the alkali remains pure: the fulphate requires for that purpofe a red heat; the nitrate only 300° of Fahrenheit: it does not detonate on ignited coals; nor does it decrepitate, or become luminous, when thrown on them.

It is difficult to faturate this alkali with carbonic acid, as it lets it efcape in the ufual temperature of the atmofphere: when faturated with it, it affumes the form of a light earthy falt.

This alkali exercifes an action on vegetable colours. It precipitates metals and earths from their folutions in acids, and in the fame manner as the other alkalies. It produces no change on *mercurius dulcis*, but it gives to corrofive fublimate the colour of carmine. It precipitates the nitrate of mercury black. When combined with oils, it forms a foap which diffolves in alcohol. Lime, precipitated by it from muriate of lime, is foluble in diftilled water. It does not decompofe muriate of ammonia but at a heat of 100° of Fahrenheit.

METEOROLOGY.

During the violent ftorm on Sunday the 9th of November laft, the mercury in a barometer at Walthamftow, Effex, was very vifibly agitated; the vibrations up and down were perhaps about four hundredth parts of an inch. This vibration was obferved by feveral perfons, and was feen at different periods of the day, during which the mercury rofe very conderably.

The fame circumftance was obferved in other places.

THE

PHILOSOPHICAL MAGAZINE.

JANUARY 1801.

I. *An Eſſay to illuſtrate the Principles of Compoſition as connected with Landſcape Painting. By Mr.* EDWARD DAYES.

To the Editor of the Philoſophical Magazine.

SIR,

AS landſcape painting is at preſent a very faſhionable purſuit, the following eſſay on compoſition may not prove unacceptable to ſome of your numerous readers; and ſhould ſuch further obſervations on the arts as I may have leiſure to offer, be deemed of importance enough to find a place in your valuable Magazine, they ſhall be at your ſervice.

I am, with much reſpect,
Sir,
Your obliged humble Servant,
EDWARD DAYES.

——————————O! attend,
Whoe'er thou art whom thoſe delights can touch,
Whoſe candid boſom the refining love
Of Nature warms,
And I will guide thee to her fav'rite walks,
And teach thy ſolitude her voice to hear,
And point her loveliеſt features to thy view. AKENSIDE.

General Obſervations.

IT will be found on inquiry, that the principles that govern one part of the arts extend to every other, whether the ſub-

ject be landſcape or hiſtory: this much it may be neceſſary to premiſe, as it indicates the dependence of one part on another as to the forming a whole. By an inquiry into the obſtructions to the obtaining a knowledge of this elegant and pleaſing part of the arts, we ſhall find it ariſe (as in moſt other caſes) from want of knowing what plan to purſue, and often from improper inſtructions; much of our ſucceſs depending on being once in a right road.

Many are taught to believe that by copying parts they will be able to form a whole; and this error is, in ſome meaſure, encouraged by many of our publications. Few things are likely to prove ſo injurious as purſuing petty plans; to dare much is the character of genius; and, if we muſt fall, let us, at any rate, fall like Phaëton.

The means moſt likely to enable us to acquire a knowledge in the arts is, firſt to ſtudy pictures, and then reſort to Nature; remembering to add to the ſtock we may collect from the wiſdom of others, ſuch original matter as may reſult from our own diligence. But though copying pictures may be neceſſary, very little knowledge will be obtained by it, other than what depends on the mechanical parts of the art. He will at all times copy beſt, who paints beſt; nor can we hope ever to become great by merely imitating another: by ſuch a practice we may learn how to mix colours, but that is very different from a knowledge of colouring. It is true, we muſt reaſon from cauſe to effect; but that is a mode of inquiry ſeldom purſued by the mere copyiſt.

Compoſition embraces two conſiderations: firſt, as it reſpects alterations which may take place in a view, and which is by the artiſt termed compoſing it; and, ſecondly, as it applies to works of fancy purely. But as the principles of one regulate the other, all that will be neceſſary is briefly to ſtate that no licenſe ſhould be taken with the view ſo as to affect the general features: diverſifying maſſes of earth, agreeably breaking the foreground, or the accidental introduction or omiſſion of any inferior object, is allowable. The forms of mountains, ſhould they appear diſagreeable, may be thrown in ſhade, or involved in clouds, in part, to conceal them; and the ſhadows may be artfully introduced to produce an agreeable

agreeably-ſhaped maſs of light, though the objects themſelves are unpleaſant.

Though we may be thus confined in treating a view, there will be ample latitude for the diſplay of our taſte in the formation of the clouds, trees, light, and ſhade, and in the diſpoſition of the animated objects. One thing highly neceſſary in the introduction of figures is, that they enter into and make part of the ſcene; and not come in as mere accompaniments, or as having no connection with the reſt of the picture. This error is daily practiſed, and argues a moſt futile imagination. A man and woman talking, a ſolitary ſailor with a bundle at his back, or miſerable fiſherman, with now and then a cow or two to keep each other in countenance, form the utmoſt ſtretch of ſome people's fancy. By a little reflection we ſhall avoid ſuch abſurdities, and be enabled to introduce our little group with fitneſs. As all ranks of perſons inhabit the country, it admits of the utmoſt diverſity in the figures; any degree of elevation or delicacy may be given, if accompanied by an appropriate employ. If the ſubject is paſtoral, though the figures need not be Arcadian, the low and vulgar ſhould be carefully avoided: it is our duty to raiſe, not depreſs, the human ſpecies: though our purſuits are humble, they need not be mean.

One thing neceſſary to the acquirement of excellence in this (or, indeed, any other branch of ſtudy) is, to think it an object of ſufficient conſequence to deſerve all our attention: this will prevent our falling into a careleſs habit, and, of courſe, going from bad to worſe. Lord Cheſterfield's obſervation ſhould never be loſt ſight of—"What is worth doing, is worth doing well." To think meanly of the arts, is to want the means to become excellent. Let us guard againſt a common error, that genius cannot exiſt unconnected with diſſipation: the fact is, the moſt renowned artiſts have been the moſt temperate. Intemperance and ſtudy cannot exiſt in the ſame mind, or at leaſt in ſuch a degree as to produce any ſenſible advantage. He who begins his career of life in the gratification of his corporeal pleaſures, will in time find the memory of all other delights deadened, and ultimately

ſink into a torpor, from which it will be impoſſible to rouſe himſelf.

> When this the watchful wicked wizard ſaw,
> With ſudden ſpring he leaped on them ſtraight;
> And ſoon as touch'd by his unhallow'd paw,
> They found themſelves within the curſed gate,
> Full hard to be repaſs'd, like that of Fate. THOMSON.

The figures in the buſtling ſcenes of Vernet are highly appropriate, and will be well worth conſulting; and thoſe in the landſcapes of N. Pouſſin are excellent examples of the higher ſtyle. Gainſborough appears to be the only inſtance of the true paſtoral that this country has produced, and is well worthy our attention for the figures.

Materials.

In treating of compoſition as it relates to works of fancy, it will be found to involve an inquiry after proper objects, and putting them together ſo as to form a picture.

By proper objects is meant the moſt perfect of their kind, accompanied with an application the moſt judicious. And here begins one of our greateſt difficulties, the diſcovering what is proper, as it embraces an extenſive field of action: whoever has acquired a knowledge of what is right, is in a fair way to do what is right. The foundation of all taſte is general inquiry, or an inquiry after the ſpecies; for, though all trees are green, and thoſe of the ſame genus reſemble each other, and though rocks and mountains bear a particular form, yet ſome are confeſſedly ſuperior, and ſhould be carefully ſelected; for *painting is not the art of imitating Nature merely*, but requires the aid of reaſon in chooſing the moſt perfect of her works, and rejecting her deformities.

> He that brings fulſome objects to my view
> (As many old have done, and many new)
> With nauſeous images my fancy fills,
> And all go down like oxymel of ſquills.
> ROSCOMMON.

This principle of general nature equally extends to colour and every other part of the art, a knowledge of which can only

only be obtained from Nature through the medium of art; that is, by carefully attending to the different excellencies found in the works of the moſt eſteemed maſters, and diligently comparing them with Nature. By ſuch a practice we ſhall in time be enabled to feel their beauties, and then we may conſider ourſelves in a fair way to poſſeſs the like. It is not a ſlight or ſuperficial view of an eſteemed picture that can benefit us; we muſt fix our mind ſteadily on it till we have, as it were, analyſed it, or diſcovered the cauſe of each particular effect, as the only means to enable us, in our future operations, to work on ſimilar principles.

With a view to aſſiſt our inquiry, it may not be amiſs to point out the peculiar excellencies to be found in the works of ſome of the moſt celebrated landſcape painters. N. Pouſſin, for dignity, will be found highly deſerving attention; his buildings and figures are unequalled. Titian's colouring is rich, accompanied with great freedom of hand and fine forms of trees. G. Pouſſin's mountains are grand, and generally form a fine line of horizon, with a peculiarity in the deep parts of the picture, and depth of water truly grand. The eccentricities of Salvator Roſa will furniſh an example of great union of parts as to chiaro-ſcuro, colour, compoſition, and figures, while his rocks are ſublime and grandly formed; the whole accompanied with great freedom of penciling. The colouring of Claude is fine, accompanied with a lovely tone of air; but his compoſitions often appear ſtudied; or, in other words, over-laboured, from the introduction of too many beautiful parts. Wilſon's compoſitions are grand, with a tone of colour truly *Titianeſque*, and a light and ſhade unequalled.

The paſtoral excellencies muſt be ſought among the works of the Dutch and Flemiſh maſters, particularly in thoſe of Rubens, whoſe colour and chiaro-ſcuro are fine; Teniers jun. who, for firmneſs of touch and the true ſilvery tone, ſtands unrivalled; Cuyp, for a light elegant touch, fine colour, and cattle; P. Potter for cattle, and, in his beſt pictures, a deep tone of colour; both the latter for a light elegant pencil, exquiſite warm amber colour, and elegantly formed trees; Beghem and Woverman's for animals, with a long ſtring of et-

cæteras: Canaletti's buildings are boldly handled, with a fine tone of colour. To enumerate the excellencies found in the different maſters were endleſs; theſe hints may ſerve to direct our inquiry.

It would be unpardonable to paſs over the merits of our countrymen Gainſborough and Barrett (not to mention many juſtly celebrated living artiſts, whoſe works will ever rank among thoſe of the firſt maſters). The former, for lightneſs of handling, elegant ruſticity of figures, breadth of *chiaro-ſcuro*, and ſweet ſilvery tone of colour, is highly worthy of attention; while the latter, for the character of a tree and the true tone of gray diſtance, is highly eſtimable. The three great names of Wilſon, Gainſborough, and Barrett, form a ſchool for the ſtudent, while their labours reflect the higheſt honour on our nation.

Some author calls painting a *ſixth ſenſe*; it certainly adds a delight to the exiſtence of the artiſt, by enabling him to enjoy many beauties unnoticed by the common eye.

> ——————For him the Spring
> Diſtills her dews, and from the ſilken gem
> Its lucid leaves unfolds: for him the hand
> Of Autumn tinges every ſertile branch
> With blooming gold. AKENSIDE.

Thoſe beauties, as they eſcape common obſervation, ſo it will become our bounden duty not to ſuffer them to paſs without making ſuch memorandums as may ſecure them for future uſe: unleſs we do this, and reſort frequently to Nature for our materials, we ſhall fall into a habit of repeating ourſelves, and our pictures will appear as if made up from the ſame ſmall ſtock of ideas. It becomes an indiſpenſable duty with us to view every thing with an eye to the art; from the palace to the cottage, from the craggy rock to the humble bank of earth: the various kinds of trees, with each ſpecies of ſhrub, muſt not paſs unnoticed. To the artiſt every thing is of uſe; beautiful or terrific, awfully vaſt, or elegantly little; all, all muſt be treaſured up for future uſe: but ſtill in our reſearch, not the individual, but the ſpecies, muſt form the object of our inquiry.

The

The higher ſtyle of landſcape is by ſome termed the *heroic*, to diſtinguiſh it from the paſtoral *. Among the objects of which the former is compoſed may be conſidered temples, pyramids, ruins of antient palaces and caſtles, altars, &c.; mountains covered with ſnow or involved in clouds, hanging rocks, and huge blocks burſting, as it were, out of the earth, &c. &c. Of objects for the latter, cottages, cloſe woods, with open views of champaign countries, &c. &c. may be noticed. Theſe can only be conſidered as making the general features; a further information will depend on induſtry.

To act is as neceſſary as to think: he who ſpends a life in comparing the ſtyles of different maſters, their peculiarities of colour, effect, or the propriety of their compoſitions, may in the end find himſelf a mere critic, but will never raiſe himſelf to the rank of an artiſt. Great advantage will be derived by frequently comparing our works with the labours of others, which will give us cauſe to rejoice at our improvement; or, what is a great ſtep towards it, diſcover our defects.

Combination.

In forming compoſitions it will not be ſufficient to bring together materials only; this can, at beſt, ſerve only to indicate a fertility of invention; they muſt be combined in ſuch a way as to preſerve an unity in the whole. Imagination is ſhown in the production of materials, but to arrange them requires the ſoundeſt judgment. To make all the parts of the picture tend to excite but one emotion will require the utmoſt care. If the ſcene to be deſcribed is ſolemn, no lively or fantaſtic image can be admitted: on the contrary, if the agreeable is intended, every thing gloomy or ſad ſhould be rejected. The neceſſity of this union of parts is equally underſtood by the poet and painter, as the following quotations from Milton will evince: ther is the utmoſt unity of parts in each, though tending to produce different ſenſations.

> Right againſt th eaſtern gate,
> Where t e great ſun begins i is ſtate,
> Rob'd in flames and amber light,
> The clouds in thouſan l nverie dight,

* There is a third, a ſort of mixed ſtyle, which does not deſerve particulariſing, as it is compoſed of the other two.

While

While the ploughman, near at hand,
Whiſtles o'er the furrow'd land,
And the milkmaid ſingeth blythe,
And the mower whets his ſcythe,
And every ſhepherd tells his tale
Under the hawthorn in the dale. L'ALLEGRO.

How animated, how lively is the whole effect, particularly when contraſted with the following:

Till civil ſuited Morn appear,
Not trick'd and frounc'd as ſhe was wont
With the Attic boy to hunt,
But kerchief'd in a comely cloud,
While rocking winds are piping loud,
Or uſher'd with a ſhower ſtill,
When the guſt has blown his fill,
Ending on the ruſtling leaves,
With minute drops from off the eaves. IL PENSEROSO.

It is evident from the above examples that figures, colour, and *chiaro-ſcuro*, muſt all have one tendency, or the picture can never form a complete whole.

We need not wonder at that want of information in the higher walks of art which at preſent pervades ſociety, if we conſider the want of knowledge in thoſe who make a trade of teaching, and that of the number of drawing-books poured on the public. Some conſider neatneſs an object; others, touch, the form of a tree; or uſher forth, as examples of art, incorrect ſketches to copy; all being content with offering a part, none teaching the combination of a whole; or that to embody a grand idea is the higheſt point of human intellect. We are in duty bound to exert ourſelves to improve the national taſte by every fair and honeſt means; and, ſhould we be ſo unfortunate as not to ſucceed, we may be comforted by the recollection that to labour to obtain excellence is excellent, as well as to endure whatever may happen to be the reſult.

Many deny the utility of the arts, while others acknowledge them as remotely uſeful only; but this were to queſtion whether ſight be preferable to blindneſs, ſenſe to folly, or life to death. As we exiſt in the ſenſes, to give them a

keener reliſh through the medium of the ſciences, is truly Epicurean.

The beſt method to improve and elevate our thoughts will be, by frequently contemplating the moſt noble objects in nature, and taking every opportunity of viewing pictures the moſt likely to inſpire fine ideas. But we ſhall view fine works of art to very little purpoſe, if we feel no higher wiſh than mere imitation: if the noble enthuſiaſm of rivalſhip does not poſſeſs our breaſts, it is much to be feared our remarks will be cold, and our exertions languid: let us then, like Jacob, dare to wreſtle even with an angel.

Various ſchemes have been recommended to aſſiſt the powers of imagination. One adviſes contemplating the breaks in the plaſtering of old walls; another, the veins of marble; and a third, as the *ne plus ultra*, has produced a ſyſtem of blotting: but thoſe methods, however ingenious, are fallacious. Gainſborough is ſaid to have formed landſcapes on the table with broken ſtones, dried herbs, and pieces of looking-glaſs; and Chattelain, whoſe drawing he was fond of, uſed to deſign his rocks from lumps of coal.

This is a bad practice. If we do not reſort to nature for our materials, and connect our inquiry with the beſt works of art, our compoſitions muſt not be expected to riſe above the paſtoral character of thoſe of the former, and our rocks, like thoſe of the latter, may ſmell of the coal-hole.

The Dutch painters, in their local repreſentation, have copied each object as it occurred, without attempting to improve them; which many imagine gives them a certain natural air, which, by the bye, is abſurd, and argues a great want of taſte: this error equally attaches to the Flemiſh ſchool, and may be ſeen even in the landſcapes of Rubens. The univerſal affection for landſcape painting does not ariſe from the love of imitation merely; the paſtoral ſcenes of the Dutch delight from other motives, and principally becauſe familiar to every imagination; they exhibit a life of peace, leiſure, and innocence, with joy, plenty, and contentment; bleſſings not to be found in the buſtling ſcenes of active life. One rule we are bound to obſerve in the paſtoral; that is, not to repreſent ſcenes of wretchedneſs, or ſuch objects as

may diſguſt. In compoſing ſcenes of rural life, though they do not require any great elevation of thought, or extraordinary capacity in the arrangement of the parts, yet they demand the greateſt care in the finiſhing, cleanneſs and delicacy in the colouring, and the utmoſt unity and ſimplicity throughout the whole. To give intereſt, we ſhould add all that ſtock of leſſer graces dependent on ourſelves; ſuch as a light elegant touch, beauty and cleanneſs of colour, and a graceful ſimplicity of form. Scenes that do not intereſt from themſelves muſt be made to do ſo by the labours of the artiſt; but where the ſubject is grand, we ſhould clothe it with all the dignity of art, accompanied with a broad, firm, and ſpirited handling.

The beautiful, in painting, as in poetry and muſic, is calculated to move the ſofter paſſions; therefore every thing abrupt and hard ſhould be rejected in the forms of the objects, chiaro-ſcuro, or colour; as is inſtanced in Claude's beſt pictures. But where a ſtronger emotion is to be raiſed, the images, forms, colour, and light and ſhade, by poſſeſſing contrary properties, will conſpire to excite oppoſite ſenſations, as in the works of Salvator Roſa, and ſome of Wilſon's grand compoſitions.

Abrupt, angular, and dark objects aſſociate beſt with the ſublime, accompanied with a certain degree of obſcurity and depth of colour.

Violent paſſions of the mind are ever accompanied with actions more or leſs angular; on the contrary, beauty loves the eaſy ſweeping line of grace, with perſpicuity, light, and a richneſs of colour: in fact, one effect ſhould, as much as poſſible, be calculated to excite painful, the other agreeable, ſenſations. Mountains involved in clouds, and objects ſeen through a miſt, will always appear with more dignity than if diſtinctly viewed.

All agreeable ſenſations are founded in temperance: too great a quantity of light, colour, or ſound, excites pain. It is the temperate of eating, drinking, ſleeping, nay, of every thing, that makes them delightful. The intemperance of Alexander cauſed him to weep for more worlds to conquer—Horrid!

Maſſy and dark ſkies will beſt aſſociate with the grand, while

while the more light and fleecy will ever attend the beautiful.

As lines are ſtrongly expreſſive of motion and paſſion, perhaps in a boiſterous ſky the forms ought to interſect each other more angularly than in a tranquil ſcene; for, as a ſtraight line is indicative of reſt, every departure from it muſt imply motion; therefore the nearer the forms, in croſſing each other, approach to a right angle, the more expreſſive of violence. But we muſt uſe caution in the practice. The ſlowly gliding river excites the idea of reſt in its ſtraight lines, in oppoſition to the contorted ones in the ruſhing current: the violent motion of lightning is always in acute angles. It is nearly the ſame whether the eye or the object moves: if the ſight is employed in tracing lines running abruptly in contrary directions, it will convey the idea of violent motion, though it is not the lines but the eye that moves. The eaſy ſerpentine ſweeping lines, expreſſive of the meandering of rivers or roads, ſerve wonderfully to carry off the diſtance. Of this there is a fine example in the picture of "Going to market," by Rubens, at Buckingham-houſe. The ſame thing, if uſed with caution, in the formation of the clouds, will carry the eye into the diſtance, and help the deception.

Thoſe who have not the opportunity of frequently applying to pictures, will find their advantage in a good collection of prints; but they muſt not be uſed to ſteal from (which is beneath the dignity of a great and independent ſpirit), but to ſtudy from, to acquire a knowledge of compoſition and chiaro-ſcuro.

An indifferent original compoſition will always be ſuperior to one patched together with ſtolen materials. The arts would be unworthy our purſuit, were they of eaſy attainment; we ſhould therefore (if we wiſh to attain excellence) be careful not to ſuffer ourſelves to be robbed of our time through an indolence of ſpirit; ſomething ſhould be done every day, if we expect ſucceſs. He who feels a deſire to act, ſhould be prompt to embrace the golden offer: if we neglect the opportunity to-day, we ſhall feel leſs inclined to-morrow; nor are we ſure at any other time we ſhall be able to rekindle the ſame ardour. To act is far eaſier than to

ſuffer; let us therefore huſband that time, the continuance of which is ſo uncertain, and whoſe loſs is irrecoverable.

> Be wiſe to-day; 'tis madneſs to defer;
> Next day the fatal precedent will plead:
> Thus on, till wiſdom is puſh'd out of life. YOUNG.

In arranging the materials of our picture, all that can be recommended is, to avoid regular forms in the maſs of objects; as angular, circular, or any geometrical figure. That regularity which conſtitutes a beauty in buildings, would become a deformity in landſcape. Should a long line of horizon, or any other objects, occur in a view, to diverſify it, ſome part ſhould be left obſcure, if it does not admit of breaking; and in compoſing the clouds, and light and ſhade, we have an opportunity of helping the effect by giving them a ſlight tendency to a contrary direction: few things appear ſo unpleaſant, as tireſome long horizontal lines following each other. But, however deſirable variety may be, we ſhould be careful to guard againſt running into an affected contraſt; a worſe evil of the two. We muſt be cautious that our compoſition does not crowd too much into one part of the picture; but, by way of ſupporting a ſort of balance, ſome one maſs, as a counterpoiſe, ſhould appear in another part: this is not confined to the objects merely, but equally extends to *chiaro-ſcuro* and colour, to prevent a ſpottineſs. It is a fault not uncommon to have too many parts in the compoſition: this ſhould be avoided in elevated ſcenes, whoſe parts ſhould be ſimple and little decorated: much finiſhing would deſtroy the ſimple dignity ſuch ſubjects require. The back-grounds to ſome of Sir Joſhua's pictures are in the firſt ſtyle of landſcape painting.

It will be our duty to diveſt ourſelves of prejudice as much as poſſible in viewing works of art: if we become partial to one maſter, we loſe the benefit we ought to derive from them all: and let us by no means adopt the conduct of thoſe who view old pictures to find out their excellencies, and modern ones their defects. Painting, as before obſerved, is not, what many ignorantly ſuppoſe, the art of copying Nature merely: no, no; it requires the aid of reaſon, and ſtrong

reaſon,

reaſon, to judge of what is fit for the art, and that it is which makes it an art indeed: a trifling ſkill will enable a man to become a mere imitator. It muſt be obſerved, however, that if our compoſition does not riſe above common nature, it will be leſs intereſting than a more indifferent local ſcene, fitted to increaſe our topographical knowledge.

From the whole of our inquiry it reſults, that the mind ſhould never ceaſe from its purſuit after whatever is beautiful or grand; let us then, by an abſtract inquiry, endeavour to create a nature of our own, if poſſible, more dignified and noble than the one that ſtrikes our ſenſes: we ſhould feel an enthuſiaſm in our purſuits not to be ſatisfied with any perfection ſhort of divine.

II. *Letter from Profeſſor* DE CARRO *to Dr.* PEARSON, *on the Vaccine Inoculation.*

DEAR SIR, Vienna, Oct. 22, 1800.

I CANNOT wait any longer to communicate to you my further ſucceſs in the vaccine inoculation. A dreadful epidemic ſmall-pox has given this autumn a new luſtre to that practice, which, from the ſenſation it has created in this city, is not likely to be ever laid aſide.

This extenſion of practice, which hitherto has been entirely, or nearly ſo, in my hands, has enabled me to eſtimate the accuracy of your obſervations on the inefficacy of a cow-pock that comes too rapidly to its height, and which does not follow the laws of that diſeaſe. I muſt candidly confeſs, that until this autumn I had not a very exact idea of the vaccine puſtule; that is to ſay, I knew very well how it ſhould be to correſpond with the deſcription of the Engliſh inoculation, ſuch as I had ſeen it on my own children; but I was not aware that it muſt be abſolutely ſo, and that every appearance after inoculation was not to be looked upon as a true vaccine. The aſtoniſhing likeneſs of my vaccine puſtule in my ſixty laſt inoculations has given me a true knowledge of it, and I ſcarcely believe that I can in future be miſtaken.

On

On the third or fourth day, one perceives a ſlight elevation and redneſs; it increaſes in a veſicular form till the 12th or 13th day, and contains always the moſt limpid fluid: a fever comes on the eighth or ninth day, or ſometimes is not perceptible: the beautiful areola appears on the ſeventh day, and increaſes till the cruſt is entirely formed: this cruſt begins commonly on the twelfth day, and becomes quite black.

I have had ſome caſes originating from count Mottet's matter, where a patient had received the ſmall-pox by inoculation, and three by natural contagion: one of theſe has retaken the vaccine in its perfect form. Luckily for the ſucceſs of the inoculation, the parents of theſe children were adviſed previouſly of the idea I had of the imperfection of their former vaccina; and they were not ſurpriſed at it, after having had a great difficulty in believing that the ſuperficial cruſt that it had formed would be ſufficient to preſerve them from future infection.

I have had two caſes where the children were already infected with the ſmall-pox at the time of the vaccine inoculation, and where it ſhowed itſelf, as uſual, without interrupting in any degree the courſe of the cow-pock. One of them proved exceſſively mild; the other is ſtill dubious.

I have had two caſes where the inoculated ſpot gave ſigns of inflammation only on the twelfth day, and where the puſtule has been perfectly regular and complete. That was the ſame ſubject that had been ſeven months ago inoculated with the imperfect cow-pock.

I have not yet ſeen any puſtule either of the *fugitive* or of the *variolous*-like appearance, although, according to Dr. Woodville's obſervations, they ſhould appear more commonly where the ſmall-pox is epidemic. There never was, perhaps, ſo diſaſtrous an epidemic at Vienna as that we have now. It is certainly owing to it that people think ſo much now of the cow-pock, againſt which they have been exceedingly incredulous. I hope to have it in my power to continue it all winter. By the laſt accounts I have of the progreſs of the vaccine inoculation at Geneva, it is exceedingly rapid. Some eruptive caſes have lately been obſerved there; but I need not enter into theſe particulars, as you will have them immediately

immediately from Doctors Petchier and Odier, or from the *Bibliotheque Britannique*. I have had lately two opportunities to make experiments upon perſons who have *undoubtedly* had the ſmall-pox in their infancy. They are both medical men, and conſequently their teſtimony is more admiſſible than that of moſt others. Upon the one, the vaccine matter produced no effect whatever; and upon the other, a very ſmall ſuperficial veſicle appeared, which changed ſoon into a cruſt. In one word, there was not the leaſt appearance of the true and uſual cow-pock puſtule. They confirm *your principle, that one cannot have the cow-pock after the ſmall-pox.* One ſingle circumſtance vexes me in my vaccine practice: it is the frequency of inoculations that fail; that is to ſay, where no puſtule nor inflammation is produced. I can ſcarcely attribute it to my method, which is the common puncture of moſt practitioners. I make it as ſuperficial as I can, to avoid, as much as poſſible, to draw blood, which I believe can waſh off the matter. I make one puncture on each arm. I ſee that in England, as well as abroad, inoculators are divided upon the advantages of the puncture or of the inciſion. You and Dr. Jenner ſeem to make uſe of the puncture: Dr. Woodville recommends ſtrongly the inciſion. At Geneva they prefer the inciſion; and here the puncture. I propoſe now to make comparative trials; and to inoculate my firſt twenty patients in one arm with the puncture, and in the other with the inciſion. The difficulty of making the moſt ſuperficial inciſion without drawing blood, has hitherto induced me to recommend the puncture. Upon the whole, I diſlike much to be ſo often under the neceſſity of repeating the inoculation. You will oblige me very much by favouring me with ſome particulars upon this point.

My ſecond patient, where the two diſeaſes have gone on together, has had alſo a very favourable ſmall-pox. Two ſimilar caſes in England authoriſe you to believe that the cow-pock can render the ſmall-pox milder when they attack the ſame ſubject together.

The medical world is waiting with impatience for the work you have promiſed in your *Statement*; and I join my requeſt to thoſe that muſt already have been addreſſed to you upon

upon the publication of a work that must throw the greatest light on a subject which engages now the attention of all civilised nations. Whenever my materials shall be sufficient, I purpose writing a complete treatise on the subject for the use of the continental physicians, and such persons as are not in the way of collecting from the various sources of information which a little zeal and activity have opened to me.

I have the honour to be,
Dear Sir,
With the sincerest esteem,
Your most obedient humble servant,
J. DE CARRO.

P. S. I sent lately some vaccine matter to Lord Elgin at Constantinople for the inoculation of his only son.

III. *Letter from* JOHN BRANSON, *Esq. Surgeon, to Dr.* PEARSON, *on the Vaccine Inoculation.*

DEAR SIR, Doncaster, Nov. 23, 1800.

IN my last letter to you I mentioned my ideas respecting *the insusceptibility of the vaccine contagion to a person who had previously had the small-pox*. I am still of the same opinion, as I have never yet been able to produce the true vaccine pustule in a person who had before undergone the small-pox; but I have lately had a case from which it would appear that, if both diseases are introduced into the system about the same time, they will both be received, and proceed through their regular stages, without being at all affected by each other. A child was inoculated for the cow-pock, after it had been for some days exposed to the contagion of the natural small-pox, on Friday the 26th of September. The arm went on in the usual way till Wednesday evening following, when the child was seized with fever, and in the evening of the next day the small-pox appeared. The eruption was confluent, and particularly so about the inoculated part, on Monday the 6th of October, which was the eleventh day from the inoculation of the vaccine matter, and the fifth from the commencement of the fever of the small-pox. I

inoculated a child from the cow-pock puſtule ſurrounded by a cluſter of ſmall-pox; in ſhort, they appeared ſo blended that I had my doubts whether the matter I obtained was that of the cow-pock or mixed with ſmall-pox; indeed I was rather diſpoſed to think the latter was the caſe, as it did not appear ſo limpid as it uſually does in the cow-pock. However, the reſult is, that it produced the genuine cow-pock without any appearance different from what I have always ſeen; and with matter from this ſource I have continued to inoculate ever ſince, without any variation either in ſymptoms or appearance. If this caſe furniſhes you with any thing new on the ſubject, I ſhall be very glad. I have no time now to make any comment upon it, as the poſt-office is juſt going to ſhut. I am, Yours ſincerely,

JOHN BRANSON.

P. S. Since I wrote laſt to you, I have inoculated upwards of 500 for the vaccine diſeaſe, without any unpleaſant ſymptoms.

IV. *Report of* C. NOWELL, *M. D. of Boulogne, Correſpondent of the Committee of Medicine, commiſſioned to repeat at Paris the Experiments reſpecting the Vaccine Inoculation, to* C. MASELET, *Sub-Præfect of the Diſtrict of Boulogne-ſur-Mer* *.

YOU are deſirous of knowing the reſults which I have obtained reſpecting the new inoculation known under the name of the *vaccine*. As it was you who firſt patroniſed it, when Dr. Woodville, Dr. Aubert, and myſelf, introduced it into France†; and as it was you who triumphed over that coalition of prejudices, intereſts, and paſſions, which had armed againſt it even the ſupreme authority, you ought to be more intereſted than any other perſon in the ſucceſs and propagation of a diſcovery which, without your aſſiſtance, would have been rejected, together with the veſſel that brought it to your fellow-citizens.

* From *Decade Philoſophique*.

† This is not true. The Vaccine Inoculation was introduced into France by the London Vaccine Inſtitution.—See the Letter of the *Ecole Medicale* to Dr. Pearſon, in our Magazine for Auguſt 1800, p. 279, &c.

Since the month of September, when I returned from Boulogne, I have inoculated for the vaccine 160 ſubjects of all ages, but the greater part children. All theſe different experiments, which were attended with the greateſt ſucceſs, have invariably diſplayed that character of mildneſs which eſtabliſhes, in an inconteſtable manner, the ſuperiority of the new over the old mode of inoculation; and the difference which we have had an opportunity of remarking in ſome of the reſults tends ſtill further to confirm this ſuperiority.

All the children and adults whom I inoculated during the whole courſe of the experiment continued their uſual ſports and exerciſes without feeling the leaſt inconvenience. The adults followed their daily occupations, and in general experienced a very ſhort ſenſation of uneaſineſs, and at moſt a ſlight febrile affection.

Six weeks after my firſt experiments I made ſucceſſively ſeven counter-experiments. I carried to the houſe of C. Beauſoleil, whoſe children were attacked with the natural ſmall pox, the eruption of which was very abundant, three children of C. Beugné, who had been inoculated with the vaccine. After leaving them a long time together, I inoculated my three ſubjects, as uſual, with the variolous matter taken from the youngeſt of the children, who was covered with the eruption; but none of them experienced the leaſt effect from the operation.

The other four ſubjects, whom I afterwards ſubjected to a counter-experiment, were, a young domeſtic of C. Wynne, another of C. Bell, and two ſiſters of the latter. I carried them to the houſe of C. Trudin, broker, whoſe child had the natural ſmall-pox. They were all inoculated without the leaſt effect.

C. Magnier, of Pont-de-Brique, having ſent for his ſon from Boulogne, where he was at ſchool, I inoculated him with the vaccine in the country. He was then ſent back to ſchool, where the ſmall-pox had broken out during his abſence. The young man continued as uſual to frequent the company of his ſchool-fellows infected with the contagion, to play with them, to eat with them, and to ſleep in the ſame room with them, without experiencing any effect.

I have

I have had an opportunity in the courſe of my experiments to be convinced that the vaccine inoculation can have no effect on thoſe who have had the ſmall-pox. C. Beugné, already mentioned, believed that he had never had the ſmall-pox. I inoculated him three different times with the vaccine matter, but always without any effect. Being ſtruck with this phænomenon, I aſked him ſome queſtions, when he acknowledged, that at the age of four years he had ſlept with one of his ſiſters who had been attacked with the confluent ſmall-pox highly malignant, that he had been very ſick for ſeveral days, but that his mother did not think him infected with the ſmall-pox becauſe he had no puſtules, which are the uſual ſymptom. This was an excellent opportunity for trying a new ſort of counter-experiment: I inoculated C. Beugné with the ſmall-pox, but they took no effect.

The ſon of C. Genou, of Samer, aged nine years, had been inoculated for the vaccine with freſh matter and of a good quality. Four days after I went to examine the child, and was much ſurpriſed to obſerve that the vaccine had produced nothing, though the child had been inoculated in three different places. This experiment had ſeldom failed, except when the ſubject had before had the ſmall-pox. On examining the child, I found him with a languid look, a bad pulſe, and a great deal of fever. I ſuſpected, therefore, that he had been already infected with the ſmall-pox at the time when I inoculated him for the vaccine. On inquiring, I found that the child had come from a houſe where the natural ſmall-pox prevailed, in order to be inoculated. I foreſaw that he was going to have the natural eruption, and indeed the diſeaſe appeared on him two days after. It was of the mild kind, and the punctures made for the vaccine were dry, and almoſt effaced.

My experience proved to me alſo, that in the caſe where the ſubject is already infected with the ſmall-pox, the vaccine inoculation, which then remains without any apparent effect, adds nothing to the malignity of the ſmall-pox. I am convinced that, whichever of the two infections is anterior in point of action and progreſs, prevents the action and pro-

greſs of the other; and conſequently, as theſe two principles cannot exiſt together, the one cannot add to the other.

Another very important obſervation, which appears to me to prove that the vaccine is really a ſpecies of the ſmall-pox, which differs from the human ſmall-pox only by its origin and the peculiar character of mildneſs by which it is diſtinguiſhed, is as follows:—A child of C. Billier, ſalt-refiner, on the ninth day of inoculation for the vaccine, which is the critical day, cut four teeth, two of them eye-teeth. A ſecond criſis of nature ſo violent, ought to have been, and was accompanied with, a ſtrong acceſs of fever, which, added to the very moderate acceſs ariſing from the vaccine, brought out on the child's body an eruption of 262 large puſtules filled with limpid matter. No common inoculation ever produced any of ſo beautiful a kind; they ended without the leaſt fever, and diſappeared at the end of ſeven days without leaving any marks. This child ſince inoculation has enjoyed perfect health; and I have remarked that is invariably the effect of the vaccine. The caſe is far from being the ſame with the common inoculation. Of the children whom I inoculated with the vaccine, ſeveral were weak and ailing, often attacked with a violent cough, and two of them were waſted with a periodical fever. All theſe children, and in particular the two laſt, have enjoyed ſince the operation perfect health.

The vaccine inoculation ſerves me as a criterion to diſſipate the uneaſineſs of thoſe who have need of being aſſured that they have had the ſmall-pox, and that they have nothing to fear.

I have offered a premium for every poor perſon who can be proved to me to have had the ſmall-pox after being inoculated, with care, for the vaccine. I ſhould not be afraid to riſk my whole fortune at preſent, on this head. But I cannot too ſtrongly recommend to practitioners to beſtow more care on this operation, and to repeat it if the leaſt doubt ſhould remain; otherwiſe there will be ſome danger of ſeeing perſons attacked with the ſmall-pox after being ſuppoſed ſecured from that diſeaſe by the vaccine. It is to errors of this kind that the obſtacles oppoſed to the introduction

duction of the common inoculation in the north of England were to be aſcribed.

This ineſtimable diſcovery has triumphed over all obſtacles as well as over every prejudice in England. Sophiſms have been refuted by facts; and it is contrary to the rules of good logic to reply to facts by hypotheſes. The celebrated Doctor David, of Rotterdam, wrote to me in the month of October laſt, that the experiments which he repeated with the matter ſent from Boulogne had been attended with the moſt complete ſucceſs; and Dr. Jenner, the author of this noble diſcovery, informed him at that period, that more than 50,000 perſons have been already inoculated with the vaccine in England; that a third of that number had been expoſed to every teſt poſſible without the ſmall-pox ever taking effect: and, in the laſt place, for five years, during which time he has been conſtantly employed in repeating the experiments, no one has ever yet refuted this theory, or weakened the concluſions he has drawn from it.

J. M. NOWELL, M. D.

Boulogne-ſur-Mer,
Dec. 3, 1800.

V. *A ſhort View of the new Electrical Experiments performed by Dr.* VAN MARUM.

[Concluded from Page 193.]

7. WHEN the conductor at the tower of Siena was ſtruck by lightning, ſeveral perſons, beſides the common electric light, clearly obſerved on it a regular train of light; and Beccaria gave to this phænomenon, which he endeavoured, without ſucceſs, to imitate with his electric machine, the name of *divergent electricity*. Mr. Landriani requeſted Dr. Van Marum to try an experiment of the ſame kind with his large machine. Both theſe philoſophers conſidered this divergent electricity as an effect of the reſiſtance which metals oppoſe to the influx of the electric matter when they have too ſmall a diameter, and throw off at the ſides the matter not received. To aſcertain this fact, an iron wire, $\frac{1}{75}$ of an inch in diameter, was placed at ſuch a diſtance from the

the conductor, that ſparks were drawn from it almoſt without interruption. The operators now obſerved with pleaſure that this ſmall wire, though in complete connection with the conductor, was covered along its whole length with a continued ſtream of light, and that this light conſiſted of ſmall rays, which iſſued from the wire on all ſides. The ſmaller the wire, the broader was the ſtream of light. Wire of other metals, of the like diameter, exhibited the ſame phænomena.

8. *Experiments to try whether the evaporation of vegetables during their being electrified would be increaſed when the pots containing them were inſulated and brought in connection with poſitive conductors.*—After being expoſed to electricity for a quarter of an hour, the loſs of weight by evaporation was found to be in ſome 1-4th, and in others 1-3d more than in the unelectric ſtate. It is here poſſible, however, that there may be ſome deception; for, as the electric conductor, when not made exactly round, always emits a wind, ſo muſt the points of the leaves of plants, and conſequently more evaporable matter muſt be carried off with it. In order to obtain ſomething deciſive on this ſubject, plants without leaves ought to be employed; but theſe evaporate very little.

9. *The influence of electricity on the ſenſitive plants.*—This was proved by Dr. Van Marum on the *Mimoſa pudica*. He firſt expoſed the plant to the ſun, in order that its leaves might be more expanded, and then applied it at the diſtance of two feet from a conductor poſitively electrified, and then to one negatively electrified. No effects, however, were produced. Being placed on the conductor, the ſmall leaves raiſed themſelves up a little and expanded, when no ſparks were drawn from the conductor; but as ſoon as ſparks were drawn from the conductor, the ſmall leaves again fell nearly in the ſame manner as the pendulum electrometer. After a few ſuch changes they at length began to approach each other, to cloſe themſelves up, and to become totally ſhut. This reſult took place alſo in conſequence of other kinds of ſhocks, and therefore is not to be aſcribed excluſively to electricity. In other reſpects, the plants experienced no other change.

In

In experiments of the same kind made with the *hedysarum gyrans*, the electricity produced no acceleration nor retardation in the movement of the small leaves.

10. Electricity produced *variations in the state of the barometer*, but only in a small degree when the mercury had not been boiled, and when, of course, it still contained some moisture.

11. An experiment was undertaken at the request of Volta, *to ascertain whether the evaporation of liquors at the common temperature of the atmosphere would be increased by electricity*. —Doctor Van Marum first placed water, alcohol, sulphuric ether, accurately weighed in small porcelain cups, on a conductor, and at the same time similar quantities at some distance from the machine. After being exposed to electricity for half an hour, no signs of increased evaporation could be perceived. The case was the same in other experiments where the circumstances were changed. Volta proposed also two other experiments; one for the purpose of trying whether electrified air be more impregnated with water than other air, and, whether the atmosphere be rarefied by electricity.

12. *On the reduction of metallic oxides by electricity*.—As this was effected in the year 1785 with sparks drawn from a battery, Dr. Van Marum was desirous of trying whether it could be done also with sparks from a conductor; whether air was produced during each revival; and if so, of what kind. For this purpose he employed tubes like those already described in art. 5. The sparks were three inches in length. Red lead was almost immediately revived, and, within twenty minutes, there were produced about $\frac{3}{4}$ cubic inch of gas, 1-3d of which was carbonic acid gas. The residuum showed in the eudiometer not so much decrease as atmospheric air. White lead, treated in the same manner, was reduced in a less degree; less air also was produced; but in other respects it appeared to be the same. Oxide of tin was not reduced, and, after being electrified half an hour, gave no air. The case was the same with the oxide of iron. Oxide of mercury prepared by heat was very soon reduced into the form of very small globules, of a black colour, adhering to

the ſides of the veſſel. The quantity of air produced, however, was ſo ſmall, that it could not be examined.

13. *Experiments for examining the quality of the air in the hall in which the machine was worked.*—For this purpoſe Sauſſure's electrometer was employed, but, inſtead of a pointed piece of metal, the flame of a wax taper was employed to receive the electricity, according to Volta's experiment. The air in this hall was of moderate dryneſs; and it was obſerved that, for the ſpace of five minutes, during which the electricity was continued, the whole air was electric, though the hall was 60 feet in length, 30 in breadth, and 40 in height. The balls ſeparated from each other at the remoteſt places half an inch; the conductor was poſitively electrified, and the air of the hall was electrified in the ſame manner.

14. Volta entertained an idea that the ſtrength of the conductor would be increaſed if its length were increaſed. Experience, however, proved the contrary. Sparks drawn from a conductor 60 feet in length and 4 inches in diameter, were five inches ſhorter than thoſe drawn from a common conductor. The ſparks ſeemed to be a little ſtronger, but they followed each other more ſlowly.

15. In repeating the *experiments on the communicating heat to bodies by electricity*, Dr. Van Marum conceived the idea of tranſmitting the ſparks through ſemi-conductors, in order to give them more energy. For this purpoſe he placed a wooden rod, one inch in thickneſs and 11 inches in length, between the ball of the conductor and the conducting wire. The conſequence was what he expected; for a rod of red fir, after being electrified three or four minutes, gave ſenſible ſigns of heat; and a thermometer, ſunk into a hole made in it, roſe in three minutes from 61 to 88 degrees, and, in five minutes, to 112 degrees. As the ſparks, however, often penetrated under the ſurface of the rod, it at length ſplit at the end, and continually threw out rays ſideways, ſo that it imitated the effects of lightning.

16. *Phoſphorus electrified in vacuo* produced a gas by which the column of mercury in half an hour fell four inches;

inches; after which it became ſtationary. In a darkened room, the electricity in vacuo appeared of a greeniſh-yellow colour. In the middle, where the electric ſtream was ſtronger, as well as at the ſurface of the phoſphorus, the rays appeared of a very lively red colour. After the paſſage of the electric current, the light did not continue a moment; but the air produced retained its elaſticity till the day following. As ſoon as a little atmoſpheric air was introduced to it, the whole ſpace above the mercury appeared luminous. Phoſphorous gas was in all probability produced. The quantity, however, was too ſmall to be examined, though of the ſame kind as that deſcribed by Gengembre in the *Journal de Phyſique* for 1785.

17. *Experiments with a battery containing 550 ſquare feet of coating.*—This battery conſiſts of 100 jars, each twelve inches in diameter, and from 22 to 23 inches in height. They are coated to within four inches of the mouth, and ſtand at the diſtance of half an inch from each other in four boxes of equal ſize. The boxes are five inches from each other, and are connected at the top by four braſs rods, and at the bottom by four plates of lead. In the middle jar ſtands a perpendicular tube, furniſhed with a ball ſix inches in diameter, and 24 holes, into which the tubes of the other jars, an inch in diameter, are inſerted: the lower ends of theſe tubes are inſerted in the balls of each jar. In the jars themſelves there are wooden rods placed on ſtands with peculiar ſupports, and on theſe braſs rods are ſtuck, ſo that nothing is cemented to the jars. When this battery had been charged by 98 turns of the machine (100 turns made it diſcharge itſelf ſpontaneouſly, by which means the jar where the diſcharge took place was pierced through,) an iron wire, $\frac{1}{10}$ inch in diameter and $24\frac{1}{2}$ inches long, was thrown about to a great diſtance in ſmall red-hot globules. By a compariſon with former experiments of this kind, it appeared that the ſtrength of the machine, by improvements made in the cuſhion, had been increaſed five-fold. Dr. Van Marum conſiders the fuſing of an iron wire, of a certain length and thickneſs, as the ſureſt means of aſcertaining the ſtrength of the ſhock of a battery. In one experiment, by a ſhock of the

above kind, a ſimilar iron wire, 36 inches in length, was thoroughly ignited, ſo that it became blue, and ſoon oxydated at the ſurface. On this occaſion, a phænomenon never before obſerved took place. At the moment of charging, the whole ſurface was covered with an exceedingly lively light, that could even be obſerved in the open day, and which in darkneſs appeared to be an inch in diameter. The ſhock alſo was much ſtronger than uſual.

A diſcharge on pieces of quartz rounded the corners and points very perceptibly, and even fuſed ſome parts of the ſtone.

18. *Experiments with the battery to aſcertain the cauſe of death in thoſe ſtruck with lightning.*—This cauſe is generally aſcribed to the irritability of the muſcular fibre being deſtroyed. Now, as animals hitherto have never loſt their lives immediately by electric ſhocks, but have been lamed or thrown into convulſions, it appeared doubtful whether an electric ſhock could actually deprive the muſcular fibres of their irritability. For this reaſon Dr. Van Marum in his experiments employed eels, which, as is well known, even when cut into three, four, or ſix parts, and when deprived of the head, ſtill retain ſigns of irritability. Theſe eels were a foot and a half in length, and the ſhock was conveyed through the whole body. By theſe means they were inſtantly killed, and never moved afterwards. They were immediately ſkinned, and trial was made, by pinching, pricking, &c. whether any irritability remained; but no traces of any were perceptible even when pretty large ſparks were drawn from theſe parts. The ſtrongeſt ſalts were attended with as little effect.

When the ſhock was made to paſs through individual parts, for example the head, theſe only loſt their irritability, while the reſt retained it. When the head was kept free from the ſhock, the remaining parts only were lamed. The ſame experiments were ſeveral times repeated on eels of $3\frac{1}{2}$ feet in length, and with the ſame reſults. When the ſhock was made to paſs through the upper and fore-part of the head of large eels, the under-jaw, as well as the muſcles of the neck and belly, and even the lower part of the body, retained their irritability, while it was completely deſtroyed in the parts

through

through which the ſhock paſſed. The ſame effects were produced in warm-blooded animals, for example rabbits, with much ſmaller batteries. Now, as, in conſequence of ſuch derangement, circulation of the blood can no longer take place, this circumſtance, without all doubt, is the cauſe of the ſudden death of thoſe ſtruck with lightning. If the ſhock does not paſs through the large arteries the animal may ſtill recover, provided the cerebellum and ſpinal marrow be not injured.

19. *The effects of ſuch ſhocks from a battery on trees.*—Nairne, in the year 1773, made experiments of this kind on different plants, particularly myrtles and laurels. Dr. Van Marum, for his experiments, made choice of much more vigorous trees, ſuch, for example, as the young ſtems of a common willow-tree, and at a period, *viz.* the middle of April, when the young branches uſually ſhoot forth. Through two of theſe, eight feet in length, he conveyed ſhocks, the firſt through a ſpace 15 inches in length, and two others through their top. After the experiments theſe trees were planted; but the parts through which the ſhocks had been conveyed ſent forth no branches. The upper parts through which the ſhocks had paſſed, ſent forth a few ſhoots for ſome days, but very ſlowly, and which ſoon after died. Thoſe not electrified ſent out branches like the other trees planted near them. The effects of electricity, therefore, are ſimilar to thoſe of lightning.

20. *Experiments on conductors.*—Dr. Van Marum had concluded from his former experiments that a rod of lead muſt be four times as large in diameter as one of iron to withſtand lightning in the ſame manner. Theſe dimenſions, however, are ſufficient for the ſtrongeſt lightning. It appeared alſo that copper conductors are equal to thoſe made of iron, even when their diameter is only one-half of that of iron ones. This appeared alſo from Brooks's experiments, who concluded that a ſlip of lead four inches in breadth, and ſo thick that a ſquare foot of it amounts to eight pounds, cannot be hurt by lightning. The experiments made by means of the large battery reſpecting the conducting property of copper, gave very different reſults. As an iron wire

$\frac{1}{45}$ of an inch in diameter and 36 feet in length, was reduced to a ſtate of ignition by a charge from 98 turns of the machine, Dr. Van Marum made a charge of the ſame kind to paſs through a copper one $\frac{1}{73}$ inch in diameter, and ſaw, with aſtoniſhment, that this wire was fuſed into ſmall globules. Another copper wire, $\frac{1}{80}$ inch in diameter, was broken in two places from a like charge; a third, of $\frac{1}{53}$ inch, remained whole. The cauſe of this difference, after a cloſe examination, was found to be in the difference of the purity of the copper. In the laſt experiments the wire was of common copper, but in the former they had been drawn from an ingot of purified copper, like that prepared to be mixed with gold. Now, as conductors are made of common copper, in determining their ſtrength attention muſt be paid to the laſt experiments, and their diameter muſt amount to half that of iron conductors, in order to preſent an equal reſiſtance to lightning. It is here ſuppoſed that the length is always the ſame. As it has been found that ſquare iron rods of half an inch in thickneſs can withſtand the ſtrongeſt lightning, in regard to copper ones, it will be ſufficient if each ſide be about four or five lines.

Dr. Van Marum faſtened the wire, through which he made the ſhock to paſs, on a piece of baked fir-lath, and found that it was burnt in ſome places where it had touched the bends of the wire. Another time he bound agaric of the oak round the wire, ſo that it lay faſt; and the conſequence was, that it took fire along its whole length. Conductors therefore which paſs along fir, or wooden work, muſt be made ſomewhat thicker than would otherwiſe be neceſſary.

Patterſon has recommended plumbago for the ſummits of conductors, becauſe it cannot be fuſed by lightning, but a charge of the large battery always reduced the ſtrongeſt plumbago to powder. If conductors are to be made pointed, they ought to be furniſhed with ſeveral points, that, in caſe one of them ſhould be fuſed, the reſt may be in a ſtate to conduct the lightning. But it appears from former experiments that pointed conductors do not deſerve that preference over blunt ones which ſome have given to them.

21. *Con-*

21. *Continuation of the experiments on the oxydating of metals; on this occasion, the semi-metals.*—As the semi-metals could not be drawn out into fine wire, thin plates of them were employed; but this could be the case only with zinc and bismuth. After the explosion the metal was seen to rise up like a thick vapour, leaving traces of it on the paper; but it could not be converted into ignited globules. Purified antimony, pulverised and strewed in a line, was oxydated, and exhibited the same phænomena as zinc and bismuth; but a larger quantity was converted into powder before it could be oxydated. Some semi-metals were mixed with so much tin that they could be drawn out into wire $\frac{1}{3^{\frac{1}{2}}}$ of an inch in diameter; for example, $\frac{1}{3}$ zinc and $\frac{2}{3}$ tin, $\frac{1}{3}$ cobalt and $\frac{3}{9}$ tin, $\frac{1}{2^{\frac{1}{5}}}$ bismuth and $\frac{2}{3^{\frac{4}{5}}}$ tin. In their oxydation, however, they exhibited nothing particular; they always rose in vapour, and left spots on the paper like the unmixed metals. A platina wire, $\frac{1}{7^{\frac{1}{3}}}$ inch in diameter, made by Jeanety at Paris, exhibited the same phænomena in fusing as silver. The platina was strewed about in fine powder, which left on the paper traces almost similar to those of iron.

22. The so-called residuum after the explosion was, with a charge of 5°, double of what it was by a charge of 15°.

23. Nairne found that, with a battery of 50 square feet of coating, the jars were readily broken, when the charge was made to explode by too short a discharging-rod, and he considered one of five feet in length sufficient to prevent this accident. Dr. Van Marum found this length sufficient for 135 feet of coating, but not for 225 feet; and, for 550 feet, a discharging-rod of 18 feet in length was not sufficient, for even then the jars were sometimes broken. None of the jars however were hurt when the communication was not made through the strong iron wire of the battery, but through very fine metallic wires, through imperfect conductors, animals, and the like, where the stream found more resistance. In charging large batteries it is necessary that the discharging-rod should receive the stream from the middle of the battery, for, when it was received on one side, two jars broke in succession on the opposite side with a charge of 20°. Brooks

says

ſays that the breaking of the jars may be prevented if paper coating be applied under that of the metal. Dr. Van Marum found this to be true, but obſerved at the ſame time that the coating was thereby weakened.

VI. *Account of ſome intereſting Experiments, performed at the London Philoſophical Society, reſpecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a Number of Gems and other refractory Subſtances ſubmitted to its Action; with a Deſcription of the Apparatus employed.*

[Concluded from Page 266.]

IN the experiments that have been deſcribed, no account was taken of the quantity of oxygen gas expended on each, as the objects the Society had chiefly in view were:

1. To fuſe, if poſſible, ſome of thoſe refractory bodies which had before reſiſted every effort hitherto made to bring them into a ſtate of fuſion;

2. To aſcertain whether any or which of the gems would retain their original colour, tranſparency and hardneſs, after fuſion, with the view of determining whether it would be poſſible by ſuch a proceſs to unite ſmall ſtones or fragments into one maſs poſſeſſing the properties of the native unfuſed gems;

3. To determine whether any other gems beſides the diamond could be diſſipated by a ſtrong heat; and,

4. To obſerve what other effects might be produced by the powerful agents employed in the experiments.

The oxygen gas employed was obtained from oxyd of manganeſe by expoſing it to a ſtrong heat in an iron retort, and was received over water in a pneumatic apparatus. The orifices of the blow-pipes were about a ſixteenth of an inch in diameter, and the gas was diſcharged under a preſſure of about three pounds weight on the bell of the gaſometer.

The view of the apparatus (Plate IX. fig. 1.) will convey a very correct idea of the gaſometer, and of the arrangements made for the laſt experiment, *viz.* the one upon platina.

The tub ABCD, three feet high and fixteen inches and a half internal diameter, is made of mahogany, and refts on a low ftand. A large glafs receiver E, open at the bottom, nearly fills the diameter of the tub. This receiver is twenty-two inches in height from the lower edge to the crown, about fixteen inches internal diameter, and is capable of containing about ten gallons of gas: it afcends or defcends in the tub, guided by the rod FG attached to its top, and paffing through the crofs-bar HI. In this crofs-bar there is fome fimple wheel-work worked by the pulley at L (concealed in the crofs-bar), and which, turning an index, tells the quantity of gas expended from time to time, when that circumftance is wifhed to be noted. The pulley at L is worked by the chain KL attached to the top of the receiver. From the pulley at L this chain paffes to the pulley M, where it is wound up or let off at pleafure by means of weights put into the fhell N, or removed thence to the horizontal plate O, which, by means of a focket, may be either put on or taken from the top of the rod FG. This contrivance is for the purpofe of enabling the operator to command the preffure of any required column of water which the tub can contain, which is therefore made deeper than the receiver. To facilitate this, the pulley M has two grooves on its circumference, one of which receives the chain KL, and is equi-diftant from the centre all round; the other, which receives the cord to which the fcale N is attached, is not equi-diftant from the pivot, but in a fnail form, fo adjufted as, with a given weight in the fcale N, to counterpoife exactly the glafs receiver E, whether wholly or partially immerfed in the water in the tub; fo that, when no weight is on the round plate O, the height of the water within and outfide of the receiver will be on a level; in other words, no preffure will be exerted on the gas it contains. By then putting known weights on the plate O, the receiver is preffed down in the water fo as to maintain an uniform preffure of any required column of water during the whole time of the procefs.

The gafometer is charged in the following manner:—Water is firft poured into the tub ABCD (till full if neceffary); the receiver of courfe afcends (no weight being then

on

on the plate O), filled with atmoſpheric air: at its greateſt height its mouth is exactly on a level with the mouth P of the tube P, Q, R, S, bent at right angles at Q and R, and joined at the end S to the tube *ab*, each end of which is likewiſe bent at right angles and furniſhed with a ſtop-cock. The tube TU, fitted air-tight to the end *b* of the tube *ab*, can be taken off at T without deranging the end U. This being taken off, and the ſtop-cock *b* being opened, a communication is made between the air in the room and that in the receiver; which being now preſſed down, either by the hand applied, or weights put on at O, diſcharges the air it contains at the opening at T; the place of which thus becomes ſupplied with water up to the top of the internal tube at P, which almoſt touches the upper part of the receiver. If the experiment require that the portion of atmoſpheric air contained in the tube PQRS ſhould be alſo removed (as when oxygen and hydrogen are to be combined in the formation of water), all that is neceſſary is to pour water into the tube, through the opening T, till all the air be driven out of the tube, and its place be occupied by water up to the opening T. Fill alſo the tube TU with water. By this means the receiver and the tubes are entirely deprived of atmoſpheric air. To clear the tubes from water, that the oxygen gas (or any other, according to the intention of the experiment) may find no obſtruction when the receiver is wiſhed to be charged, a large tin flaſk, having a neck ground to fit the cock Q, and charged with oxygen gas, is joined to the cock Q, which muſt then be opened: the gas aſcends from the flaſk, and the water in SR and PQ takes its place. The cock U muſt then be opened, to allow the water in the tube TU to deſcend into the caſk V.

Oxygen gas is then to be transferred to the receiver from a caſk V, previouſly filled with it. The cock *a* is kept ſhut, and *b* opened, and water is made to deſcend from the funnel W into the caſk by turning the cock *c*. The water, of courſe, diſplaces an equal volume of gas in the caſk, and forces it through the tubes UT, *ba*, and SRQP into the receiver E, diſplacing the water contained in it. While the gas is paſſing into the receiver, the funnel W muſt never be allowed

allowed to run itſelf empty, otherwiſe the atmoſpheric air getting into the bottom pipe, a portion of it might be forced down with the next ſupply of water. The receiver being charged with gas, ſhut the cocks *c* and *b* for the preſent.

Should any experiment require more gas than the receiver will contain, the ſtream may be kept up direct from the caſk by opening the communication UT *ba*, and taking care to keep the funnel W well filled with water. Another caſk may be joined to the firſt by a mode of communication ſo obvious that we need not deſcribe it; ſo that oxygen gas may eaſily be ſupplied for a length of time ſufficient to melt down completely the whole internal arrangements of any furnace.

In none of the experiments, except the 32d, were the table-furnace and its appendages employed. In the firſt ſeven, a ſimple blow-pipe was joined to the tube of the gaſometer, and the ſubſtance to be operated upon, laid on a piece of ignited charcoal, was expoſed to the iſſuing ſtream of gas. It was obſerved in the courſe of theſe experiments that the ſubſtances ſeemed ſometimes to be partially cooled, by the ſtream falling on them while the charcoal was moving about to expoſe the different parts round the ſtone to the action of the gas. To obviate this inconvenience, the double blow-pipe already mentioned was conſtructed, the nozles of which formed ſuch an angle as to make the ſtreams of blaſt croſs each other. From this it is obvious, that the charcoal placed at any diſtance from the nozles ſhort of that where the ſtreams would croſs, received two ſtreams of gas; and that the nearer the charcoal was brought to the blow-pipe, the wider was the ſpace between the centres of the ſpots on which the blaſt fell. By this means it became eaſy to expoſe the gems and other ſubſtances to the full action of the caloric liberated by the decompoſition of the oxygen gas, without putting them in the way of the undecompoſed ſtreams.

This double blow-pipe is repreſented in fig. 2. The part A, which joins the tube of the gaſometer, turns air-tight in a collar B, on the end of the tube BC. On BC are two braſs boxes *d*, *e*, into which are fitted the tubes *f*, *g*, which alſo turn in ſockets air-tight for the purpoſe of enabling the operator to move the blow-pipes *b*, *i*, nearer or

further from each other. The blow-pipes *h, i*, alſo turn at *k, l*, in the tubes *f, g*, to enable the operator to alter the direction of the ſtreams, and make them fall on the charcoal *m*, at any angle he pleaſes. The double blow-pipe juſt deſcribed was that employed in all the experiments, from the 8th to the 31ſt inclusive.

VII. *Extract of a Letter from Dr.* SAM. L. MITCHILL, *Profeſſor of Chemiſtry in Columbia College, to Mr.* TILLOCH.

SIR, New-York, Dec. 3, 1800.

. . . . . CONSIDERING all things, I really truſt that the weſtern world is doing its part in the philoſophical work of the day. We have great and excellent opportunities of obſerving phænomena here, and ſome among us are diligent to let no opportunity be loſt. The following points we think fully eſtabliſhed:

1. There is proof as pointed as it needs to be, that the ſickly ſeaſons which afflict our cities are accompanied with, and occaſioned by, noxious exhalations, *locally* produced either on board ſea-veſſels, or in ſtores, cellars, dwelling-houſes, and ſinks, in our towns.

2. When exiſting in ſhips, theſe noxious productions are not received on board in the ports of the Weſt Indies, or other places beyond ſea, as is vulgarly believed, but univerſally are produced within the ſides of the veſſel itſelf, from naſtineſs and corruption there.

3. Not only are the exhalations from our *corrupting fiſh*, *beef*, and *hides*, and from our *abominable privy-pits*, very injurious to health, and deſtructive of life, but theſe very vapours are ſo *acid* as to be ſmelled and taſted by the repackers of proviſions.

4. In the alimentary canal of ſuch perſons as feed upon beef and fiſh, and, generally ſpeaking, of *lean animal ſubſtance*, there is formed a *ſimilar acid*, whoſe preſence has been detected, and of a ſtrength ſufficient to curdle milk, to excoriate the fundament, and to effervеſce with carbonats of alkaline ſalts.

5. The

5. The fame *acid* product, or fome modification of it, which, when volatiliſed and ſpread through the atmoſphere, produces our *en*demic fevers (for our annual diſtempers are not *epi*demics), of the various grades from intermittent to *yellow* and *peſtilential*, does, when engendered in the inteſtinal canal, ſtir up *dyſentery and its concomitant ſymptoms*.

6. The excellency of alkaline remedies, eſpecially the neutral ſalts, in which ſoda is combined with a weak acid, evinces the exiſtence of an inflaming and corroding ſourneſs, which being overcome either by alkalies *per os* or *per anum*, gives the patient great and ſpeedy relief.

7. The application of the ſame mode of reaſoning to *the human mouth*, which has been employed with reſpect to the ſtomach and bowels, will explain the generation and noxious effect *of a ſimilar acid among the teeth* and around the gums, of their corroſion and deſtruction by it, and of the utility and importance of alkalies as dentifrices and ſweeteners of the mouth.

8. An explanation is, on the ſame principles, given to the manner in which *human garments grow foul and peſtilential*, the excretions lodged in them degenerating *to acidity by expoſure to the air*, and thus becoming *fomites of infection*. This infection never poſſeſſing any *ſpecific* quality, but merely being the acid offspring of common putrefaction. And on this depends the theory of *alkalies, and leys and ſoaps*, in deſtroying infection if preſent, or in preventing its formation, and their wholeſome and purifying power in waſhing, ſcouring, and houſekeeping.

9. Experiments lately made in the New-York hoſpital have proved to me that *foul and ill-conditioned ulcers*, eſpecially of the *ſiphylitic* kind, contain an acid ſo conſiderable as in *three or four hours to turn litmus-paper red*. I have found *alkaline remedies* of admirable uſe in ſuch ſurgical caſes locally applied. We are thus poſſeſſed of a clue to explain much of the nature of *corroding malignant and infectious ulceration*; of the *manner of ſtopping it by alkalies*; and of the explanation of *hectic fever* from an abſorption of this *acid virus*.

10. It is rendered plain of what materials *cities ought to*

be built and paved to be moſt healthy, to wit, the *calcareous*; and that a wiſe policy ſhould introduce marble and limeſtone into general uſe.

11. The uſe of *pit-coal for fuel* has an additional recommendation. During combuſtion it affords much *ammoniac*; and this volatile alkali is capable of neutraliſing *abundance of ſeptic* acid. Commonly, where the burning of coal is general, peſtilential diſtempers are more rare than they uſe to be. But my paper fails me before I have finiſhed my enumeration: I muſt therefore conclude, and leave the reſt for a future communication, though not without aſſuring you that I remain yours, with much regard and reſpect,

To A. Tilloch, Eſq. SAM. L. MITCHILL.

VIII. *Extract of a Letter from Profeſſor* ABILDGAARD, *Secretary to the Royal Society at Copenhagen, to* C. HUZARD, *Member of the French National Inſtitute, on the Quantity of Carbon in the Blood*.*

I SHALL give you the reſult of ſome experiments which I made and repeated to diſcover the quantity of carbon that exiſts in the blood, and which gave me leſs of that ſubſtance in the arterial than in the venous blood.

1ſt, A hundred parts of the venous blood of a horſe, when dried in a moderate heat, gave 26 parts of a ſubſtance ſo dry that it could be pulveriſed.

2d, A hundred parts of arterial blood of the ſame horſe gave 25 parts of dry ſubſtance.

3d, To alkaliſe, in the manner of Kirwan, an ounce of nitre by detonation (the ounce being 480 grains), required 192 grains of venous blood, and only 160 of arterial.

4th, An ounce of venous blood, after being dried and decompoſed in a cloſe veſſel, yielded 115½ grains of charcoal.

5th, The ſame quantity of arterial blood gave only 87½ grains of charcoal.

6th, To decompoſe 480 grains of nitre, required 148 grains

* From the *Annales de Chimie*, No. 126.

of

of charcoal of venous blood; and to decompoſe the ſame quantity of nitre, required 119 grains of charcoal of arterial blood. This experiment, indeed, was not very correct, becauſe a very light part of the charcoal was diſſipated like duſt.

7th, I ſeparated the red part of the blood from the ſerum and fibrous part, as completely as poſſible, by the means commonly employed; and after drying it, I tried it by nitre. To alkaliſe 480 grains of nitre, required 130 grains of this red part of the blood.

8th, To alkaliſe by detonation 480 grains of nitre, required 202 grains of the fibrous part ſeparated from the ſerum; and yet with this part the nitre detonated more briſkly than with the other parts of the blood.

IX. *Analyſis of the Honey-ſtone, or Mellite.* By C. Vauquelin *.

THE analyſes of this ſtone given by Abich and Lampadius are well known. The former obtained from 100 parts of it, 16 of carbonat of alumine, 4 of carbon, 3 of the oxide of iron, 40 of carbonic acid, 28 of the water of cryſtalliſation having the ſmell of bitter almonds, and 5·5 of naphtha.

The latter had for reſult 86·4 of carbon †, 3·5 of petroleum, 2 of ſilex, and 3 of the water of cryſtalliſation; which makes an enormous difference.

Mr. Abich, conſidering the incombuſtibility of the mellite, propoſes to remove it from the claſs of combuſtibles, and to place it in that of the incombuſtibles. But Profeſſor Klaproth, whoſe labours are entitled to the greateſt confidence, informed me ſeveral months ago that he found this

* *Annales de Chimie*, No. 107.

† If M. Lampadius operated on the ſame ſubſtance as that which M. Abich and I analyſed, it is impoſſible that he ſhould have obtained 86·4 of carbon; for, in 40 of carbonic acid and 4 of carbon obtained by M. Abich, there was not a ſufficiency to form 86 of carbon; and as it appears from my analyſis that there is not more than 55 per cent. of real acid in honey-ſtone, it is evident that 86 of carbon cannot be extracted from it. M. Lampadius therefore muſt have operated in another manner, or did not employ heat ſufficient to analyſe the acid, if the ſubſtance he analyſed was really honey-ſtone.

pretended

pretended ſtone to be compoſed of a peculiar vegetable acid united with alumine.

Profeſſor Abildgaard, to whom I am indebted for many curious minerals from Norway, ſent me a few weeks ago, by the hands of M. Mantey, a ſmall quantity of mellite, a part of which he deſtined for the purpoſe of analyſis, and I took the earlieſt opportunity of complying with his wiſhes in that reſpect.

Deſcription of the Mellite.

This ſubſtance has a light yellow colour, on which account it has been called mellite, or honey-ſtone: it generally cryſtalliſes in octaedra, the angles of which are ſometimes replaced by facets ariſing from laws of decrement, which have not attained to their limits. Its ſpecific gravity is not conſiderable; according to Mr. Abich it is about 1·666. It is found in Thuringia in ſtrata of foſſil and bituminous wood.

Chemical Characters of Mellite.

I. When expoſed to the action of heat in contact with the air this ſubſtance becomes white, and burns without becoming ſenſibly charred: it leaves as reſiduum a white matter, which produces a ſlight effervescence with acids. It has no ſenſible ſavour, yet, if kept for ſome time on the tongue, it occaſions a faint impreſſion of acidity.

Analyſis.

II. I took two grammes of mellite, reduced to powder, and mixed them with four grammes of ſaturated carbonat of potaſh diſſolved in a ſufficient quantity of water. As ſoon as the mixture was made it produced a pretty ſtrong effervescence without the aſſiſtance of foreign heat; but to accelerate the decompoſition of this ſubſtance, and render it more complete, I expoſed it to a gentle heat on a ſand bath.

The liquor, when filtrated after cooling, had a browniſh colour, and left on the paper a brown matter, which when dried in the ſun weighed nearly 0·8 of a gramme.

III. Theſe 0·8 of brown matter, when calcined in a crucible, became white, and weighed no more than 0·33 of a gramme.

gramme. When mixed with ſulphuric acid diluted with water, they produced a ſlight effervescence: the mixture was then evaporated to dryneſs.

According to what had been announced by Profeſſor Klaproth, I expected that by the addition of water almoſt the whole of the above matter would be diſſolved; but, on the contrary, the greater part remained under the form of a white powder.

The liquor having been evaporated till there remained no more than 3 or 4 grammes, I added to it a drop of the ſulphat of potaſh, and obtained by ſpontaneous evaporation about 0·1 gramme of alum mixed with a little ſulphat of lime.

I then examined the nature of the matter, which when treated with ſulphuric acid had not diſſolved in water. For this purpoſe I boiled it with a ſolution of the carbonat of potaſh, and, when filtrated and waſhed, I had the following reſult:

1ſt. The muriatic acid, diluted with two parts of water, attacked it, exciting a ſtrong effervescence; but the ſolution did not become clear; on the contrary, it remained milky.

2d. The liquor, when filtrated, gave with ammonia a tranſparent precipitate, reſembling that ariſing from alumine by the ſame means; but it was not entirely ſoluble in potaſh. The greater part however was diſſolved by the potaſh, and exhibited all the characters of alumine; for, when combined with the ſulphuric acid, it gave alum. The cauſe therefore why this ſubſtance did not remain combined with the ſulphuric acid was, in all probability, its being too much heated towards the end of the deſiccation.

The liquor from which the ammonia had ſeparated the alumine already ſpoken of ſtill gave ſlight precipitates by the carbonat of potaſh and the oxalat of ammonia; which proves that it contained a little lime.

That portion of the matter not diſſolved by the potaſh weighed at moſt 0·1 gramme, and appeared to me to be *ſilex*. Mellite then contains a ſmall quantity of lime and ſilex.

After aſcertaining the kind of matter which compoſed my reſiduum, I then examined the liquor, which I concluded

muſt contain acid of mellite united to potaſh; and in the hope that it would give up its baſe to mineral acids, I put into a portion of the liquor a few drops of nitric acid, which produced a very ſlight effervescence, and gave birth to a ſmall quantity of a brown flaky matter. Some hours after what I ſuſpected took place; the acid of mellite cryſtalliſed under the form of ſmall ſhort priſms with brilliant facets.

Finding that this method might enable me to ſeparate the above acid from the potaſh, I expoſed the whole of the liquor to a gentle heat, and mixed with it ſome nitric acid till it had an exceſs ſenſible to the taſte. I then made it paſs through the filter, in order to ſeparate the brown flaky matter, and to obtain the acid in a purer ſtate. In two cryſtalliſations I indeed obtained about 1·34 grammes of it, which were pretty white, though it had ſtill a yellowiſh tint. The properties which it exhibited by its mixture with other ſubſtances were as follow:

1ſt. This acid has brilliant facets, a conſiderable degree of hardneſs, and a ſlight acid ſavour accompanied with a little bitterneſs, which may have ariſen from ſome particles of bitumen that remained attached to it, and which gave it a yellowiſh colour.

2d. A portion of this acid, when expoſed to the flame of the blow-pipe, exhibited at firſt ſome ſcintillations like ſaltpetre; it then ſwelled up, and left a matter which ſoon penetrated the charcoal.

3d. When heated in a covered platina crucible, it at firſt ſwelled up, then became carbonaceous without producing an oily ſmoke, and left a light charcoal which was exceedingly alkaline *. This acid remained therefore united to a certain quantity of potaſh, notwithſtanding the exceſs of nitric acid added to its ſolution. The ſame effect took place alſo with the tartareous and oxalic acids, which by theſe means paſs to the ſtate of *acidulous* ſalts.

4th. This ſalt is very little ſoluble, but I have not been

* This acid, on account of the above property, cannot be confounded with the acidulous tartrite of potaſh; for the latter ſwells up much more, and during its decompoſition emits a pretty thick ſmoke, which has a peculiar odour eaſily diſtinguiſhed.

able

able to determine exactly the proportion of water it requires.

5th. Some grammes of the ſame acid diſſolved in water, being mixed, 1ſt, with a ſolution of lime, immediately formed a white flaky precipitate, which ſoon depoſited itſelf at the bottom of the liquor: 2d, with a ſolution of the ſulphat of lime, a light granulated and cryſtalliſed precipitate, which left to the water a little tranſparency, but which was increaſed and rendered flaky by the addition of a drop of ammonia *: 3d, with a ſolution of the muriat of barytes, a very ſmall precipitate at firſt, but ſome moments after a multitude of cryſtals in the form of needles: 4th, with a ſolution of ſilver, a white precipitate, ſilky and brilliant like a ſolution of ſoap; ſome time after it depoſited itſelf under the form of duſt: 5th, with a ſolution of lead by nitric acid, a white pulverulent and very heavy precipitate: 6th, with a ſolution of mercury, a white precipitate which was rendered black by a drop of ammonia.

From the reſult of theſe experiments it appears the acid of the mellite has a great many properties analogous to thoſe of the acid of ſorrel; and by the compariſon which I made, I could perceive no other differences than the following: 1ſt, The precipitate which it occaſions in the ſolution of ſulphat of lime manifeſts itſelf leſs ſpeedily, and is cryſtalline, inſtead of being pulverulent like that formed by the acidulous oxalat of potaſh. 2d, It ſeems leſs acid to the taſte than the acidulous oxalat of potaſh, but this may be owing to my not having added to its combination with potaſh enough of nitric acid to deprive it of a ſufficient quantity of that alkali. 3d, It ſwells up a little more by heat than the acidulous oxalat of potaſh.

In a word, the ſublimated ſalt, the large quantity of car-

* The acidulous tartrite of potaſh does not immediately produce a precipitate in a ſolution of the ſulphat of lime, but in 24 hours after there are formed in the mixture cryſtals with very brilliant facets, which are a compound of lime and tartareous acid. Though cryſtalliſed, this tartrite of lime has no reſemblance to that produced by the acid of honey-ſtone with the ſame matter; it differs from it by ſwelling up in the fire, whereas the other is decompoſed without ſwelling up; and in this has an analogy to the oxalat of lime.

bonic acid, that of water, and the ſmall portion of charcoal which the mellite furniſhes by diſtillation, are all facts which ſeem to concur to prove the identity of theſe two acids; for the ſalt of ſorrel exhibits in the fire the ſame phænomena *.

The octaedral form of the mellite ſeems alſo to have an analogy with that of the oxalic acid, which is a rectangular priſm terminated by pyramids of four faces: to be certain of this, nothing is neceſſary but to compare the inclination of the faces.

However, as I had at my diſpoſal only about 1·34 gramme of this acid, I was not able to ſubject it to all the teſts neceſſary to demonſtrate, in a poſitive manner, its identity in every point with the oxalic acid; for, though they exhibited analogous phænomena in all the comparative experiments I made, it is poſſible that by others which may be made hereafter there may occur one difference ſufficient to deſtroy the reſemblance.

I have therefore publiſhed this notice chiefly with a view of inducing the chemiſts of Germany, where this ſubſtance is moſt commonly found, to repeat the analyſis of it, and to compare, under every point of view, its acid with that of ſorrel. Should my opinion be confirmed by new experiments, we ſhall then have oxalic acid in the three kingdoms of nature, *viz.* in the ſtate of acidulous oxalat of potaſh in ſeveral kinds of vegetables; in that of oxalat of lime in human urinary calculi; and, in the laſt place, in the ſtate of oxalat of alumine in the interior of the earth among bituminous kinds of wood; but, in whatever place found, it ſeems always to be indebted for its origin to vegetable matters †.

X. *Dr.*

* The acid of ſorrel, or oxalic acid, is that which furniſhes by diſtillation the largeſt quantity of carbonic acid and water; becauſe, of all the natural vegetable acids known, it contains moſt oxygen.

† Since the above paper was written, I conceived the idea of mixing the acid of honey-ſtone, united to a little potaſh, with a ſolution of pure ſulphat of alumine, and there was immediately formed a very abundant flaky depoſit: on the other hand, I put into a ſolution of the ſame ſalt acidulous oxalat of potaſh, but there were no ſigns of precipitation.

Theſe different effects, therefore, ought to excite well-founded doubts reſpecting the identity of the acid of honey-ſtone and the oxalic acid; and

I confeſs

X. *Dr.* DICKSON's *Tranſlation of* CARNOT *on the Infiniteſimal Calculus.*

[Continued from Page 240.]

The fundamental Principles of the Infiniteſimal Analyſis.

32. THEOREM I.—*If in any imperfect equation whatever, there be ſubſtituted for any one of its component quantities, another quantity differing infinitely little from it, or whoſe ratio to the firſt hath unity for its limit, or ultimate value; then, I ſay, that the equation reſulting from this transformation cannot be a falſe equation, that is, it will become abſolutely exact, or, at leaſt, will remain what I call an imperfect equation.*

For ſince, by the hypotheſis, there has been ſubſtituted for one quantity another of the ſame ultimate value, and whoſe ratio to the former hath unity for its limit, it is evident that ſuch ſubſtitution could not change either the ultimate values of the ſides of the propoſed equation, or their ultimate ratio. Now, by the hypotheſis, this ultimate ratio was (1) unity before the ſubſtitution; therefore it will ſtill remain unity; and conſequently the equation will preſerve the character of what I call an imperfect one, if it do not become rigorouſly exact. Q. E. D.

33. THEOREM II.—*An equation which contains only aſſigned quantities, cannot be an imperfect equation.*

For, by the definition of imperfect equations (article 31), their ſides are unequal, but differ infinitely little from each other, their ratio approaching, as nearly as we pleaſe, to the ratio of equality; therefore there enters into ſuch an equation ſome quantity which makes no part of the ſyſtem of the quantities propoſed. But by the hypotheſis the propoſed

I confeſs that I ſhall ſuſpend my judgment on this point, notwithſtanding the inclination which I firſt had to believe theſe two acids to be of the ſame nature. For this reaſon, before any thing can be determined, we muſt wait until experiments made on a larger ſcale threw more light on the ſubject.

equation contains aſſigned quantities only, and, conſequently, it cannot be what I call an imperfect equation. Q. E. D.

34. THEOREM III.—*Every imperfect equation which hath undergone ſuch transformations as are indicated in the firſt theorem, and from which all unaſſigned quantities have been eliminated, by thoſe transformations, will be neceſſarily and rigorouſly exact.*

For, by the firſt theorem, the equation cannot be abſolutely falſe; and, by the ſecond, it cannot be imperfect; therefore it is neceſſarily and rigorouſly exact. Q. E. D.*

35. COROLLARY.—All that hath been ſaid on the ſubject of imperfect equations, ought to be underſtood of all the propoſitions, propoſitions, and reaſonings whatſoever, which can be expreſſed and delivered by ſuch equations.

The leading Principle of this Analyſis.

36. SCHOLIUM.—Such are the general principles into which the theory of the Infinitefimal Calculus is reſolvable. From theſe principles it appears, that if, after expreſſing the conditions of a problem in imperfect equations, we arrive, by means of ſuch transformations as are indicated in the firſt theorem, at the elimination of all auxiliary or unaſſigned quantities, a compenſation of errors muſt neceſſarily have taken place, in the courſe of the proceſs. It further appears, that the advantage of the Infinitefimal Calculus conſiſts in this, That, the conditions of a queſtion being often very difficult to be expreſſed accurately by rigorous equations, it is eaſy to do it by imperfect equations, from which as certain reſults can be derived, as if the original equations had been perfectly accurate; and this by the ſimple expedient of eliminating the quantities whoſe preſence occaſioned the error.

* The word *falſe*, as before intimated (in the Note on § 9), ſeems by far too ſtrong a term to be applied, in any allowable ſenſe, to ſuch equations as the author is conſidering. He ſhould have defined it, and adhered to his definition; for, in art. 31, he uſes *falſe* and *imperfect* as convertible terms, and, in art. 32 and 33, he takes them in oppoſite ſenſes. This unſteadineſs renders his meaning ſomewhat ambiguous. But if, by falſe or imperfect equations, he uniformly mean ſuch whoſe ſides differ infinitely little from equality, then his whole meaning becomes clear, and his three theorems almoſt ſelf-evident.—W. D.

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The reason of this procedure is simple. Suppose we have occasion to investigate the relations which subsist between several proposed quantities. If it be difficult to find directly equations to express these relations, we naturally recur to some intermediate quantities, which may serve as terms of comparison. By this means we obtain, if not the very equations sought, at least other equations, in which the proposed quantities are blended with auxiliary ones; and there can be no question that these last ought to be eliminated. But if, additionally, the values of these auxiliary quantities be arbitrary, and may be supposed as small as we please, without affecting the proposed quantities, it is easy to see, that, if in the equations expressing the relations sought, arbitrary quantities be mixed with proposed ones, each of these equations may be decomposed into two, one containing assigned, and the other arbitrary quantities. It is nearly in this manner, that an equation containing real and imaginary quantities may be decomposed into two equations, the one consisting of real, and the other of imaginary quantities. Now, as we only want the equation which exists between the proposed quantities, it is evident that, in those equations where they are mixed with arbitrary ones, we may safely neglect the quantities which embarrass our calculation, when the resulting errors can only affect the equation between the arbitrary quantities which it contains. Now this is precisely what takes place in the Infinitesimal Calculus, where we consider infinitely small quantities as nullities when compared with finite ones.

In order to render this explanation still clearer, let us resume our former example. In article 9, we found

$$TP + T'T = y\frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ}.$$

These two equations are both perfectly exact, whatever be the values of MZ and RZ. Deducing, then, from the first equation, the value of $\frac{MZ}{RZ}$, and substituting it in the second, I get

$$\frac{TP + T'T}{y} = \frac{2y + RZ}{2a - 2x - MZ};$$

and this equation is accurate, as it ought to be, whatever distance

diſtance we may ſuppoſe to intervene betwixt the lines *RS* and *MP*.

Now, it is eaſy to ſee that this laſt equation is ſuſceptible of the following form:

$$\left(\frac{TP}{y} - \frac{y}{a-x}\right) + \left(\frac{T'T}{y} - \frac{yMZ + aRZ - xRZ}{(a-x).(2a-2x-MZ)}\right) = 0^*,$$

an

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* As this equation appears to be erroneous, I ſhall examine it throughout; putting, for brevity, $TP = s$, $T'T = \dot{s}$, $MZ = \dot{x}$, $RZ = \dot{y}$. The equation immediately preceding, in this notation, is

$$\frac{s+\dot{s}}{y} = \frac{2y+\dot{y}}{2a-2x-\dot{x}}, \text{ or } \frac{s}{y} + \frac{\dot{s}}{y} = \frac{2y+\dot{y}}{2a-2x-\dot{x}},$$

which being made $= 0$, is $\left(\frac{s}{y} + \frac{\dot{s}}{y}\right) - \left(\frac{2y+\dot{y}}{2a-2x-\dot{x}}\right) = 0$.

Now, (by article 9) $s = \frac{y^2}{a-x}$, or $\frac{s}{y} = \frac{y}{a-x}$, or $\frac{s}{y} - \frac{y}{a-x} = 0$; and this will be the firſt member of the new equation, which is to contain what our author calls "aſſigned" quantities only, as the ſecond member is to contain none but "auxiliary" quantities. The firſt part of the ſecond member is obviouſly $\frac{\dot{s}}{y}$, an auxiliary quantity; and, in order to render the ſecond part, $-\left(\frac{2y+\dot{y}}{2a-2x-\dot{x}}\right)$ alſo *wholly* auxiliary, we multiply both the numerator and denominator by $a-x$ (or by its equal $\frac{y\dot{y}}{\dot{x}}$, by art. 9). This multiplication by $a-x$, gives the latter part of the numerator $a\dot{y}-x\dot{y}$; and thus far the author's new equation is right.

But the auxiliary, equivalent to the aſſigned quantity $2y.(a-x)$, cannot be what he makes it; for, as we have juſt ſeen, $a-x = \frac{y\dot{y}}{\dot{x}}$, and therefore it ſhould be $2y.(a-x) = \frac{2y^2\dot{y}}{\dot{x}}$.

Now the author has $2y.(a-x) = y\dot{x}$, and conſequently, *if his reſult be right*, we ſhould have,

$$\frac{2y^2\dot{y}}{\dot{x}} = y\dot{x}, \text{ and } y = \frac{\dot{x}^2}{2\dot{y}}.$$

But (by art. 9, again) $\frac{y}{a-x} = \frac{\dot{x}}{\dot{y}}$, and $y = \frac{a\dot{x}-x\dot{x}}{\dot{y}}$,

Therefore $\frac{a\dot{x}-x\dot{x}}{\dot{y}} = \frac{\dot{x}^2}{2\dot{y}}$, and $a-x = \frac{\dot{x}}{2}$;

But this being plainly impoſſible, I conclude that $2y.(a-x) = \frac{2y^2\dot{y}}{\dot{x}}$, or $2y^2\dot{y} : \dot{x}$ $\left(= 2y^2, (RZ : MZ)\right.$ in our author's notation$\left.\right)$

and

an equation, the first term of which contains only given quantities, and the second only arbitrary quantities, which

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and that the new equation should be,

$$\left(\frac{s}{y} - \frac{y}{a-x}\right) + \left(\frac{\dot{s}}{y} - \frac{2y^2\dot{y} : \dot{x} + a\dot{y} - x\dot{y}}{(a-x) \cdot (2a - 2x - \dot{x})}\right) = 0,$$

$$\text{or } \left(\frac{TP}{y} - \frac{v}{a-x}\right) + \left(\frac{T'T}{y} - \frac{2y^2 \cdot (RZ : MZ) + aRZ - xRZ}{(a-x) \cdot (2a-2x-MZ)}\right) = 0;$$

and, in this last manner, I shall, in future, take the liberty to write it.

It the value of $a-x$ had been taken from the accurate equation (in article 9) $\frac{\dot{x}}{\dot{y}} = \frac{2y + \dot{y}}{2a - 2x - \dot{x}}$, we should then have had

$$2y \cdot (a-x) = (2y^2\dot{y} + y\dot{y}^2 + y\dot{x}^2) : \dot{x};$$

so that, *in rigid strictness*, the new equation should be,

$$\left(\frac{s}{y} - \frac{y}{a-x}\right) + \left(\frac{\dot{s}}{y} - \frac{(2y^2\dot{y} + y\dot{y}^2 + y\dot{x}^2) : \dot{x} + a\dot{y} - x\dot{y}}{(a-x) \cdot (2a - 2x - \dot{x})}\right) = 0.$$

Now, though $\dot{y}^2$ and $\dot{x}^2$ be infinitely less *still* than their infinitely small roots $\dot{y}$ and $\dot{x}$, and consequently $y\dot{y}^2 + y\dot{x}^2$ infinitely less than $2y^2\dot{y}$, and thus may be safely neglected; yet it would not, perhaps, have been amiss, if the author, while he was exhibiting all his quantities, had brought *them* also into view.

Some readers may be surprised at the mention of quantities infinitely less than infinitely small ones. But their wonder will cease when they recollect, That if any integer, or any fraction, be multiplied by a fraction, the product will be *less* than the multiplicand, and will, in fact, be only such a part of the multiplicand, as the multiplier is of unity. Thus, if ·000000000001 (prefixing cyphers *ad infinitum*) be multiplied by any other interminably or inconceivably small fraction, the product will be only such an inconceivably small part of the *already* inconceivably small multiplicand, as is expressed by the inconceivably small multiplier: in other words, the product (relatively to our conceptions) may be said to be infinitely less than the infinitely small multiplicand. In like manner, $\dot{x}y$ may represent a rectangle, whose breadth $\dot{x}$ is infinitely small compared with its length y, which may be any finite line, or it may even be an indefinitely or infinitely great line. But now suppose y also to be infinitely small, or to become $\dot{y}$; then it is easy to see, that this second rectangle $\dot{x}\dot{y}$, both whose dimensions are infinitely small, will be infinitely less than the first rectangle $\dot{x}y$, which has only one of its dimensions infinitely small. Thus also the squares $\dot{x}^2$ and $\dot{y}^2$ will be infinitely less than their infinitely small roots $\dot{x}$ and $\dot{y}$, and the cubes $\dot{x}^3$ and $\dot{y}^3$, than the squares $\dot{x}^2$ and y^2, &c.—*Nemo enim novit natura limitem.*

Were this the proper place, we might recommend these and many analogous considerations, both mathematical and metaphysical, to the serious attention of certain gentlemen, who, without abating a tittle from their high pretensions to accurate reasoning, scruple not to tell us, in very general terms, "that they cannot believe any thing which they cannot conceive or comprehend."—W. D.

last

laſt may be ſuppoſed as ſmall as we pleaſe, without affecting the quantities in the firſt term; becauſe we may ſuppoſe RS to be as near as we pleaſe to MP. Agreeably, therefore, to the theory of indeterminate quantities, each of the terms of this equation, taken ſeparately, muſt deſtroy itſelf, or be equal to nothing; that is, this equation may be reſolved or decompoſed into two others, namely,

$$\frac{TP}{y} - \frac{y}{a-x} = 0, \text{ and } \frac{T'T}{y} - \frac{2y^2.(RZ:MZ) + aRZ - xRZ}{(a-x).(2a-2x-MZ)} = 0,$$

the firſt of which equations contains only aſſigned quantities, and the ſecond none but arbitrary ones. But aſſigned quantities alone are neceſſary to our purpoſe; for they give us the required value of TP, the ſame that we before found it. When, therefore, we have even committed errors in the courſe of the calculation, the exactneſs of the reſult will not be affected, provided that thoſe errors are confined to the ſecond equation. And this, in fact, is the ſame thing which would have happened if we had conſidered MZ, RZ, and $T'T$, as nullities, in compariſon with the quantities a, x, and y. We ſhould, indeed, have committed errors in expreſſing the conditions of the problem; but theſe errors would have deſtroyed each other by compenſation, and the required reſult would not, in any reſpect, have been altered.

The Infiniteſimal Analyſis is only an Application or Extenſion of the Method of proceeding in Indeterminate Problems.

37. From what has been ſaid, it will be eaſy to perceive that the Infiniteſimal Analyſis is nothing elſe than an application, or, if you will, an extenſion of the Method of Indeterminates. For, agreeably to that method, I ſay, that when we neglect an infinitely ſmall quantity, we do nothing more, properly ſpeaking, than *underſtand* it, and do not ſuppoſe it to be nothing. Thus, when inſtead of the two exact equations, found in article 9, namely,

$$TP + T'T = MP\frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ},$$

I employ the two imperfect equations,

$$TP = MP\frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{y}{a-x};$$

I know

I know very well that I am committing an error, and I put the equations, *mentally*, so to speak, into this form,

$$MP\,\frac{MZ}{RZ} = TP + \varphi, \text{ and } \frac{MZ}{RZ} = \frac{y}{a-x} + \varphi';$$

φ and φ' being such quantities as the former equations want to render them exact. In like manner, in the equation $\frac{TP}{MP} = \frac{y}{a-x}$, resulting from the above two imperfect equations, I *understand* the quantity φ'', being such that $\left(\frac{TP}{MP} - \frac{y}{a-x}\right) + \varphi'' = 0$, may be an exact equation. But I know well enough, that this last quantity φ'' is equal to zero; or, at least, that it is only an infinitely small quantity, since no infinitesimal enters into the first term. Now this cannot happen, unless each of the terms, taken separately, be equal to nothing; whence I conclude, that I have exactly $\frac{TP}{MP} = \frac{y}{a-x}$; so that the quantities φ, φ' and φ'' have not been suppressed as nullities, but only *understood*, in order to simplify the calculation.

Again: if X, for example, be an arbitrary quantity, which may be rendered as small as we please, and if there were given an equation of this form,

$$A + BX + CX^2 + \&c. = 0,$$

A, B, C, &c. being independent on X, this equation cannot exist, unless it be $A = 0$, $B = 0$, $C = 0$, &c.; that is, unless each term, taken separately, whatever be their number, be equal to zero. And, for the same reason, if we have an equation of this general form, $P + Q = 0$; so that P may be a function of the quantities given or determined by the conditions of the problem; and, on the other hand, Q, a quantity which we may suppose as small as we please, we shall necessarily have $P = 0$, and $Q = 0$. But such is precisely the nature of the equation in the last article, namely,

$$\left(\frac{TP}{y} - \frac{y}{a-x}\right) + \left(\frac{T'T}{y} - \frac{2y^2 \cdot (RZ : MZ) + aRZ - xRZ}{(a-x) \cdot (2a - 2x - MZ)}\right) = 0.$$

Therefore each of the terms of this equation, taken separately, is equal to zero; and consequently, the quantities $T'T$, MZ and RZ, which enter not into the first term, may

be neglected, in the courſe of the calculation, without altering that firſt term.

The Infiniteſimal Analyſis, therefore, differs from the method of indeterminates only in this, that in the former, quantities which, were they allowed to remain, would, in the end, always deſtroy one another, are treated as nothing, or rather are *underſtood* throughout the calculation; while, in the Method of Indeterminates, we wait till the end of the calculation, and then cancel the arbitrary quantities which ought to be eliminated. This laſt method may therefore very eaſily be made to ſupply the uſe of the Infiniteſimal Calculus, without the help of imperfect equations, and without committing any error in the courſe of the calculation.

38. There is yet another method of coming at the reſults of the Infiniteſimal Analyſis, without overpaſſing the bounds of ordinary algebra; and that is, by the Method of Limits, or Ultimate Ratios. For though this analyſis be founded entirely on the properties of limits and ultimate ratios, it differs nevertheleſs from what is properly called the method of limits, in this, that in the latter, the quantities which we call Infiniteſimal, do not enter ſeparately into the calculation, nor even their ratios, but only the ultimate values of theſe ratios, which being finite quantities, do not ſo properly conſtitute this method a particular calculus, as a ſimple application of ordinary algebra.

The buſineſs before us, then, is by barely introducing into ordinary algebra, not Infiniteſimal quantities themſelves, but the ultimate ratios of theſe quantities, to ſupply the means which the Infiniteſimal Analyſis furniſhes, for diſcovering any properties, ratios and relations whatſoever, of the magnitudes which conſtitute any propoſed ſyſtem; and this is that which is properly called the Method of Limits.

To explain the procedure, and give ſome idea of the ſpirit, of this method, we ſhall again reſume the example before treated of.

Explanation of the Method of Limits, properly ſo called.

It is evident, from what was delivered in article 9, that, though $\frac{MZ}{RZ}$ be not equal to $\frac{TP}{MP}$, yet the firſt of theſe

quantities differs so much the less from the second, as RS approaches nearer to MP; or, in other words, that $\frac{MZ}{RZ} = \frac{TP}{MP}$ is an imperfect equation; but that (putting L for the limit, or the ultimate value,) $L \cdot \frac{MZ}{RZ} = \frac{TP}{MP}$ is a perfect, or rigorously exact, equation.

In like manner, $L \cdot \frac{MZ}{RZ} = \frac{y}{a-x}$ is proved to be a perfect, or rigorously exact, equation. Equating then these two values of $L \cdot \frac{MZ}{RZ}$, there arises, as before,

$$\frac{TP}{MP} = \frac{y}{a-x}, \text{ or } (MP \text{ being} = y)\ TP = \frac{y^2}{a-x}.$$

Thus, this new calculus contains neither the infinitely small quantities MZ and RZ, nor even their ratio $\frac{MZ}{RZ}$; but only the limit or ultimate value of that ratio, namely, $L \cdot \frac{MZ}{RZ}$, which is a finite quantity.

39. If this method could be always as easily put in practice as the ordinary Infinitesimal Analysis, it might even appear the most eligible of the two: for it would have the advantage of conducting us to the same results, by a path which is always direct and luminous; whereas the other conducts us to the truth, only after having made us traverse, so to speak, the regions of error.

But it must be owned that the Method of Limits is attended with a considerable difficulty, which has no place in the ordinary Infinitesimal Calculus. In the former, the infinitely small quantities cannot, as in the latter, be separated from each other; and these quantities being always connected two and two, afford no opportunity of introducing into the combinations, the properties of each in particular, or of subjecting the equations into which they enter, to those transformations which may assist in their elimination. This difficulty is much less felt in the operations themselves, than in the preparatory and supplemental propositions and reasonings.

The Origin of the Name, Infinitely Small Quantities.

40. From what has been ſaid (in article 2.) on the origin which the Infiniteſimal Analyſis might have had, it appears, that the quantities called Infinitely Small, received that name from it's being at firſt believed that it was neceſſary, for the ſucceſs of the calculations in which they were employed, to attribute to thoſe arbitrary quantities values, which were really leſs than any which could be recognized by the ſenſes, or conceived by the imagination. But a better digeſted theory has made it appear, that ſuch a ſuppoſition is unneceſſary; ſince the ſucceſs of this calculus proceeds not from the attenuation of thoſe arbitrary quantities, but ſolely from the compenſation of errors which they occaſion in the proceſs.

We have ſeen indeed, in illuſtrating the example ſo often adduced, that the procedure and the reſults of the calculation were preciſely the ſame, whatever value we attributed to the infinitely ſmall quantities MZ and RZ, and that conſequently the character of this kind of quantities conſiſts not in their real minuteneſs, but rather in their being abſolutely indeterminate, that is, in their property of remaining arbitrary throughout the calculation, and ſo independant on the propoſed quantities, that we can always take them as ſmall as we pleaſe, without changing, in any reſpect, the conditions of the problem.

Infiniteſimal quantities, as was obſerved in article 24, are by no means chimerical beings, but ſimply variable quantities, characterized by the nature of their limit, which is 0 for infinitely ſmall quantities, and $\frac{1}{0}$ for thoſe which are infinitely great. To theſe indeterminate quantities, as well as to all other indefinite quantities, may be ſucceſſively attributed, divers arbitrary values, and among thoſe values ought to be included the ultimate values of all, that is, 0, for quantities infinitely ſmall, and $\frac{1}{0}$ for thoſe which are infinitely great.

Diſtinction

Distinction of Mathematical Infinity into Sensible and Absolute.

41. This observation leads to the distinction of mathematical infinity into two kinds, namely, *sensible*, or *assignable*, infinity, and *absolute*, or *metaphysical*, infinity, which is the limit of the former.

If then, to any infinitely small quantity, be assigned a determinate value, which is not 0, this value will be what I call a *sensible*, or *assignable*, Infinitesimal; whereas, if this value be the last of all, that is, if it be absolutely nothing, it will be what I call an *absolute*, or *metaphysical*, Infinitesimal, which I shall also distinguish by the name of an *evanescent* quantity. Thus an evanescent quantity is not that which is generally called an infinitely small quantity, but only the ultimate value of that quantity. It is only, I say, a determinate value which, like any other value, may be attributed to that arbitrary quantity, which is generally denominated infinitely small.

42. The consideration of these evanescent quantities would be almost useless, if in calculation we were restricted to treat them as simple nullities; for, in that case, they would present only the vague ratio of 0 to 0, which is no more equal to 2 than it is to 3, or to any other quantity whatsoever. But it must not be forgotten, that these nullities are here invested with particular properties, as the ultimate value of indefinitely small quantities, whose limits they are; and that the particular epithet, *evanescent*, is applied to them in order to denote, that, of all the ratios and relations of which they are susceptible in quality of nullities, no other is considered as entering into the calculation, than those which the law of continuity assigns to them, when the system of auxiliary quantities is supposed insensibly to approach to the system of assigned quantities. This idea is what some great geometricians have thought they could express, when they said, that evanescent quantities were quantities considered, neither before nor after they had vanished, but in the very instant of their vanishing *.

For

* (*C'est ce que de grands geometres ont cru pouvoir exprimer*, &c.) The author here plainly alludes to Sir I. Newton, the author of this doctrine

For example, in the case before adduced, as long as RS does not coincide with MP, the fraction $\frac{MZ}{RZ}$ is greater than $\frac{TP}{y}$; nor do these fractions become equal, till MZ and RZ are reduced to nothing. It is true, that *then*, $\frac{MZ}{RZ}$ is as much equal to any other quantity as to $\frac{TP}{y}$; because $\frac{0}{0}$ is a quantity altogether arbitrary; but, among all the different values which $\frac{MZ}{RZ}$ may be supposed to have, $\frac{TP}{y}$ is the only one which is subjected to the law of continuity and determined by it. For, if a curve were constructed, whose abscisse

doctrine of prime and ultimate ratios, and of the whole method of Fluxions. That great man, in the concluding scholium of Sect. 1. B. 1. of the *Principia*, has these words: "*Objectio est*, &c." "It may be objected that evanescent quantities have no ultimate proportion; for that, before they vanish, that proportion cannot be the last, and after they have vanished, it is nothing. But, by the same argument, when a moving body stops at a certain place, it may be said that it has no ultimate velocity, for that, before the body reaches that place, the velocity is not the ultimate velocity, and when it has reached the place, the velocity is nothing. The answer is easy; for by the ultimate velocity is meant the velocity of the body, neither *before* it reaches its last place, nor *after* it has reached it, but that velocity with which it actually reaches it; that is, the very velocity with which the body attains its last place, and comes to rest. In like manner, by the ultimate ratio of evanescent quantities is to be understood, the ratio of those quantities, neither *before*, nor *after* they vanish, but *the ratio with which they vanish*. And thus also the prime ratio of nascent quantities is that ratio with which they first start into existence," &c.

Though but a humble and distant follower of Newton,

Quem longe sequor, et vestigia pronus adoro,

I see nothing that could hinder him from "thinking he could express" the fundamental principle of this doctrine by such language. For my own part, I must frankly say, that the scholium whence it is quoted, conveys, or suggests, that principle more clearly to my mind, than all that our ingenious author and others have written on the subject. But we do not all see things, with equal clearness, in the same point of view. Some of my superiors in genius and knowledge have a different opinion of that scholium, and of the rest of Newton's fluxionary doctrine, as delivered by himself, and even as explained by Ditton, Simpson, and others. To such I would recommend the present perspicuous tract.—W. D.

was

was the indefinitely small quantity MZ, and it's ordinate proportional to $\frac{MZ}{RZ}$, that which would answer to the abscisse o, would be represented by $\frac{TP}{y}$, and not by an arbitrary quantity. Now this is what distinguishes the quantities which I call evanescent from those which are simply nothing.

Thus, though in general we have $o = 2 \times o, = 3 \times o, = 4 \times o$, &c. yet we cannot treat an evanescent quantity, such as MZ, in the same manner, and say $MZ = 2MZ = 3MZ = 4MZ$, &c.; for the law of continuity cannot assign to MZ and MZ, any other ratio than that of equality, nor any other relation than that of identity *.

43. We have seen that by introducing into the calculation infinitely small quantities, and by neglecting them in comparison with finite quantities, the equations became imperfect, and that the errors which they produced were only compensated in the required result. But we have it now in our power to avoid this kind of inconvenience, by means of evanescent quantities, which being nothing else than the ultimate values of the infinitely small quantities corresponding to them, may, like any of the other values, be attributed to these indefinitely small quantities; and which being, in another point of view, absolute nullities, may be neglected, when they are found connected with any effective quantities, without preventing the calculation from being perfectly rigorous.

44. The Infinitesimal Analysis, then, may be considered in two different points of view; by regarding the infinitely small quantities, either as real, effective quantities, or as absolute nullities. In the first case, the Infinitesimal Analysis is nothing else than the Calculus of compensated errors; and in the second, it is the art of comparing evanescent quantities among themselves, and with others, in order to deduce from these comparisons the proportions and relations, whatever they may be, which subsist among the quantities proposed.

* For "identity," the author should have used the word "congruity." They are very far from being synonymous terms; though used as such by some mathematicians. See Euclid's 8th axiom.—W. D.

Evanescent

Evaneſcent quantities, as being equal to nothing, ought to be neglected in the calculation, when they are connected, by addition or ſubtraction, to any real, effective quantity. But they have, nevertheleſs, as we have ſeen, relations very important to be known, relations which are determined by the law of continuity, to which the ſyſtem of auxiliary quantities is ſubjected in its mutations. Now, in order to diſcover this law of continuity, it is eaſy to perceive, that we are obliged to conſider theſe evaneſcent quantities at ſome diſtance from the limit where they entirely vaniſh, otherwiſe they preſent only the indefinite ratio of 0 to 0; but this diſtance is arbitrary, and hath no other object but to enable us to judge more eaſily of the ratios or relations which exiſt between theſe evaneſcent quantities. Theſe are the ratios which we have in view, when we conſider infinitely ſmall quantities as abſolute nullities, and not thoſe ratios which exiſt between the quantities which are not yet arrived at their limit, or the term of their annihilation. Theſe laſt quantities, which I have called indefinitely ſmall, are not themſelves deſigned to enter into the calculus conſidered in the preſent point of view; but are only employed to aſſiſt the imagination, and to indicate the law of continuity which determines the ratios and relations, whatever they may be, of the correſponding evaneſcent quantities.

According to this hypotheſis, the quantities repreſented by MZ and RZ, in the proportion $MZ : RZ :: TP : MP$, are ſuppoſed abſolutely equal to nothing. But, as it is their ratio that is required, in order to perceive it's equality to $\frac{TP}{MP}$, the indefinitely ſmall quantities, which anſwer to theſe nullities, muſt be conſidered, not that they themſelves may be introduced into the calculation, but that the vaniſhing quantities, which are their ultimate values, may enter into it, under the denominations of MZ and RZ.

45. Theſe expreſſions therefore, MZ and RZ, here repreſent nullities, and are uſed under the forms of MZ and RZ, rather than under the common form 0, becauſe, if they were uſed under this laſt form, it would no longer be poſſible, in the operations wherein they are mixed, to diſtinguiſh their different origins, or, in other words, to diſtinguiſh the different

ferent indefinitely small quantities which answer to them. Now the consideration of these last, at least mentally, is necessary to the apprehension of the law of continuity, which determines the required ratio of the evanescent quantities which are their limits; and consequently it is essentially necessary to keep them in view, and to characterize them by expressions which may prevent them from being confounded.

46. Evanescent quantities, which are the subject of the Infinitesimal Calculus, considered in this new point of light, are, it is true, *entia rationis*, creatures of the understanding. But this does not hinder them from having mathematical properties, and from being compared together, as well as imaginary quantities in Algebra *, which have no better claim to existence. For it is not more certain, for example, that

$$60 \text{ is } = 20 + 40, \text{ than that } \sqrt{-a} \text{ is } = \sqrt{-b} \times \sqrt{\frac{a}{b}}.$$

Now no person doubts the accuracy of the results obtained by the calculus of imaginary quantities; though they be only the algebraic forms and emblems of absurd quantities. With much greater reason are we prevented from rejecting evanescent quantities, which are at least the limits of real quantities, and are *in intimate contact*, so to speak, with their existence. What signifies it, indeed, Whether these evanescent quantities are, or are not, chimerical entities, if their *ratios* be not such, and if *these ratios alone interest us?* When, therefore, we subject infinitesimal quantities to calculation,

* The author might have added points, lines, surfaces and solids in geometry; for they too are *entia rationis*, which have no existence in external nature. Points are merely the terms or limits of lines, lines of surfaces, and surfaces of solids; just as the prime ratios of nascent, and the ultimate ratios of evanescent quantities, are the limits of those ratios, when the quantities are considered as beginning, or ceasing, to be. Yet geometry rests on this foundation of abstract entities, with perfect security; for the truth is, that, without abstract ideas, science, *strictly* so called, cannot exist. And he who can conceive a mathematical point, a mere abstract *locus*, a something without parts, an entity truly one and indivisible, which, being a creature of the intellect, entirely eludes the cognizance of every sense; I say such a man will no sooner understand Newton's doctrine of prime and ultimate ratios, than he will esteem it a legitimate foundation of mathematical reasoning.—W. D.

we have it entirely in our power to consider them either as real quantities, or as absolute nullities. The difference between these two ways of considering this question, consists in this, that, by regarding evanescent quantities as nullities, the propositions, equations and results, whatever they may be, are always accurate and rigorous; but have a reference to quantities which are creatures of the understanding, and express the relations which exist between quantities which do not themselves exist *. On the other hand, by considering infinitely small quantities as having some reality, the propositions, equations and results, whatever they may be, have for their subject real quantities. But these last propositions, equations and results are false, or rather imperfect, and become exact in the end only in consequence of the compensation of their errors, a compensation, however, which is the necessary and infallible result of the operations of the calculus.

* Thus the ratios of the ordinal numbers (*one*, *two*, *three*, &c.) to each other, while those numbers remain floating, so to speak, in indeterminate abstraction, and unapplied to any particular objects of sense, may be said to "express the relations which exist between quantities which do not themselves exist." Thus also, if a body be *supposed* to fall from any moderate height, its velocities at any two points (resistance apart) will have to each other the ratio of the square roots of the spaces supposed to be described; although no body ever *actually* fell, or perhaps ever will actually fall, from that precise height. These examples, it is hoped, will prevent readers who are not much accustomed to such speculations, from rashly charging our author with absurdity, in talking of the relations between quantities which do not themselves exist; that is, which have no existence in external nature. For the truth is, and a surprising, unaccountable truth it appears to many beginners, that the objects of Pure Mathematics, though originally abstracted, or copied, from external objects, have no existence out of the minds which conceive them; and hence proceeds all that accuracy for which those sciences are justly valued. The inaccuracy of the figures, motions, &c. of external objects induces a corresponding inaccuracy into Mixed Mathematics.—The inaccuracy of language has an analogous effect in metaphysics; for metaphysical relations and deductions may be perfectly accurate *in the mind*, yet few of them can be adequately and unexceptionably expressed, for want of an accurate, unambiguous language. Hence the endless disputes with which men unhappily disposed to cavil, and who affect to doubt of every thing, never cease to embroil that important, and otherwise not unpleasant, region of philosophy.—W. D.

47. The

47. The theory which has been thus expounded easily furnishes answers to all the objections which have been made against the Infinitesimal Analysis, the principle of which several geometricians have thought faulty, and capable of leading to erroneous conclusions. But those gentlemen have been overwhelmed, if the expression may be allowed, with a multitude of prodigies, and with the splendour of the numerous truths wherewith this principle has teemed.

These objections may be reduced to this. The quantities said to be infinitely small, are either absolute nullities, or they are not; for it is ridiculous to suppose that there exist entities, which hold a middle place between quantity and nullity. Now if they are absolute nullities their comparison leads to nothing, for the ratio of o to o is no more *a* than it is *b*, or any other quantity whatever. If, on the other hand, they are not nullities, but real quantities, they cannot without error be neglected, as the rules of the Infinitesimal Analysis prescribe.

The answer is simple. So far from its not being logical to consider infinitely small quantities, either as real beings, or as nothings, they may, on the contrary, be treated at pleasure, either as nullities or as true quantities. For they who wish to consider them as nullities, may answer, that what they call infinitely small quantities are not any nullities taken at random, but nullities assigned by the law of continuity which determines their relation*; that among all the relations of which these quantities are susceptible as nullities, they only consider those which are determined by this law of continuity; and, in a word, that these relations are not vague and arbitrary, because the law of continuity does not assign several different relations between the differentials, for example, of the abscisse and ordinate of a curve, when these differentials vanish, but one only, which is that of the subtangent to the ordinate.

On the other hand, they who regard infinitely small quantities as true ones, may answer, that what they call an infinitely small quantity is one which is arbitrary and independant on the qualities proposed; that, therefore, without supposing

* See the Note on article 22.

it nothing, it may be treated as such, and yet no error will exist in the result; because that error, if there were any, would be arbitrary, like the quantity which occasioned it. Now it is evident, that no such error can exist, except among quantities, one of which at least is arbitrary. When, therefore, we arrive at a result containing no arbitrary quantity, and which expresses any relation whatever between quantities given, and those determined by the conditions of the problem, we may rest assured that that result is accurate; and that, consequently, the errors necessarily committed in expressing these conditions, must have been compensated and have disappeared, by the necessary and infallible effects of the operation.

48. Other mathematicians, apparently embarrassed by the objection just discussed, have simply confined themselves to prove, that the Method of Limits, the processes of which are rigorously accurate, in all respects, must necessarily lead to the same results as the Infinitesimal Calculus. But, while it is agreed that the principle of that method is very luminous, it cannot be dissembled, that the difficulty is thus only eluded, not removed; that the Method of Limits leads to the same results as the Infinitesimal Calculus, only by a difficult and circuitous way; and, in fine, that that method, far from being the same with the Infinitesimal Calculus, is, on the contrary, only the art of dispensing with this calculus, and of supplying it by ordinary Algebra. It appears to me, that they would succeed, in a more simple manner, by the Method of Indeterminates. But why adopt one of these methods to the exclusion of the rest, when they can afford us their mutual assistance? Let us then employ them all—the Infinitesimal Calculus, properly so called, the Method of Limits, and the Method of Indeterminates, as circumstances may require, and let us neglect none of the means which can conduct us to truth, or simplify our researches.

It remains for me to show, by some examples, the application of the general principles, which I have explained. This I shall do, by giving my reader an *idea* of the *Differential* and *Integral Calculi*, which, properly speaking, are the Infinitesimal Analysis itself reduced to practice.

[To be continued.]

XI. *Ac-*

XI. *Account of* C. F. DAMBERGER'S *Travels through the interior Parts of Africa, from the Cape of Good Hope to Morocco.*

[Continued from p. 253.]

ON the 15th of December our traveller proceeded on his journey, and after paſſing through three villages reſted for ſome time near a fourth under a beautiful *matabora* tree *. Here he was viſited by ſome of the natives, who offered him a piece of wolf's fleſh, which he at firſt refuſed; but finding he could get nothing elſe, he ate it, and thought it taſted better than that of the buffalo. Two miles further he found in a wood a prodigious number of tortoiſes, which exciting his appetite, he reſolved to ſpend the night there under ſome trees. He therefore made a large fire, and roaſted ſome of the fleſh of theſe animals, but ate ſo freely, that for ſome time he could not ſleep. Towards morning he fell aſleep, but had ſcarcely remained an hour in that ſtate, when he found ſomething move at his feet; upon which he ſtarted up, and diſcovered a ſnake three ells in length and a foot in thickneſs, devouring the remains of his meal. Being much alarmed at the appearance of this gueſt, he immediately haſtened from the ſpot and continued his journey. Towards evening he arrived at a narrow but long ridge of mountains, where he reſted for the night.

Next morning, when about to depart, he heard human voices, and ſoon after ſaw about twenty armed men, who were eſcorting a company of priſoners, coupled together, two and two, with thongs. Some of the former immediately ſprung towards him, and conducted him to their leader, who, after ſurveying him for ſome time, took a thong which was wound round his middle, and, having bound with it his hands and feet, ordered him to follow him. Towards noon they arrived at the large village of Cuojaha, where they reſted, and procured a ſupply of water. This was the firſt village in the

* The leaves of this tree are ſmall and long; the branches hang down, and the flowers are of a reddiſh colour. The fruit are round, and about the ſize of a large pea; they contain a kind of meal of an excellent taſte, which is formed into a ſort of cakes not inferior to biſcuit.

province of Porguhomat, which in maps is called Ofila. Damberger's fellow-travellers belonged to this nation, and reſided at a place half a day's journey further to the north.

This province lies at the diſtance of four days' journey from the boundaries of the kingdom of Congo; it is exceedingly fertile, and abounds with productions of various kinds. The people are warlike, and defend their territories with great bravery. Our traveller was told, that they could bring into the field from 18,000 to 20,000 men; but he believes this account to be exaggerated one half, as the population appeared to be only very moderate. This nation was formerly ſubject to the king of Benguela, who procured from it his beſt ſoldiers; but as theſe ſoldiers were often ſold for ſlaves when the prince ſtood in need of money, the people rebelled in the year 1776, and, putting to death the old king Khiguan, declared themſelves independent. The ſon of the murdered king was afterwards placed on the throne; but in every thing that relates to public affairs he is obliged to be guided by the advice of the oldeſt of the inhabitants, who are aſſigned to him as counſellors. Diſputes and quarrels among theſe people are ſettled by the oldeſt perſons in each family; and the offending party, when the cauſe has been determined, is puniſhed out of doors. Many of their cuſtoms are ſimilar to thoſe of the neighbouring nations, but they have ſome peculiar to themſelves. There are fewer women here than in ſome of the other African nations, and it is even not uncommon for two men to have only one wife, and yet to live together in perfect harmony. The women are highly reſpected, and treated much better than among the ſurrounding nations. Male children immediately after birth are circumciſed, and great rejoicings take place on ſuch occaſions, becauſe boys are much more valued than girls. As ſoon as a child can ſpeak, he is taught by his grandfather, or, if he has none, by his father, to know thoſe vegetables and fruits which can be uſed as food, and to guard againſt thoſe which are pernicious. He muſt learn alſo to make mats of long graſs, and, when he becomes older, to take a ſhare in the management of the domeſtic concerns of the family: his chief attention, however, is directed to hunting, in which it

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is requiſite that he ſhould diſtinguiſh himſelf by his boldneſs and dexterity. When he kills an elephant, he is no longer claſſed among the boys, but conſidered as having attained to the rank of a man. Every ſtranger not engaged in trade with this nation is conſidered as a ſlave, but he is well treated, and never afterwards ſold; for theſe people deteſt this traffic, and endeavour to free from their ſtate of ſlavery thoſe unfortunate beings who are deſtined by other nations to be ſold. When they learn therefore that any ſlave-dealers are about to paſs through or near their territories with a cargo of ſlaves, they aſſemble in a large body, attack the ſlave-merchants, and, having delivered the ſlaves, receive them as members of their nation. Our traveller was obliged to accompany a party of them on an expedition of this kind, but he had the misfortune to be taken priſoner. The people by whom he was made captive were called the Sovians.

Being liberated however ſome time after, he continued his journey on the 26th of February, 1786, and on the 5th of June following arrived at the village of Mohakam on the frontiers of the antient kingdom of Loango, where he procured a guide who conducted him to the town of Malemba, the capital of the united kingdoms of Malemba and Cacongo. This country is about four hundred miles in length, and two hundred and forty in breadth; it is exceedingly fertile, and abounds with vegetables, minerals, and animals of various kinds which are caught for the ſake of their ſkins. The river Bambo and the lake Samoy ſupply plenty of fiſh and ſhell-fiſh. The trade of this country is exceedingly advantageous to the Europeans, who for the mereſt trifles, ſuch as ſhells and bits of iron, often get in return the beſt ſkins and furs in large quantities. The king's ſtanding army conſiſts of between ten and twelve thouſand men, who can at any time be collected in the courſe of forty-eight hours. The king himſelf is a good warrior, leads his troops into the field in perſon, and is much dreaded even by his more powerful neighbours: whenever he goes abroad he is uſually attended by four of his miniſters, who are at the ſame time officers, and twelve men of his body-guard.

The town is ſurrounded by a wall compoſed of fragments

of rock and looſe ſtones, heaped together without clay or mortar, and by a ditch. The palace where the king reſides is badly built, is one ſtory high, and about five hundred feet in circumference. It ſtands before the water-gate on the river Malempo, towards the lake, and affords a good proſpect. Our traveller ſaw planted near it four three-pounders.

After being expoſed to various hardſhips among theſe people, our traveller, being ſent out with a party to collect elephants' teeth, found means to make his eſcape, and arrived among a people called the Yaganeſe, with whom he remained till the end of the year. On the 2d of December he again reſumed his journey, and, croſſing the Akaſi mountains, reached the territory of Mugari. This country is ſmall, and thinly peopled. The inhabitants are of low ſtature, and far from being hoſpitable to ſtrangers. Though of ſmall ſize, they are more expert in hunting the elephant than many of their neighbours; and from the hides and teeth of theſe animals they procure their chief ſubſiſtence. The whole number of the inhabitants does not amount to more than three thouſand: moſt of them live in caves of the mountains.

After paſſing through a variety of ſmall villages our traveller entered the kingdom of Yukodego (Monœmugi), which towards the eaſt borders on Abyſſinia. The river Zambece, which at the town of Yukora divides itſelf into five branches, traverſes nearly the whole of the country, and, at the diſtance of half a day's journey from the town of Zambre, forms a large lake, bearing the name of that town. The town of Zambre, which he viſited ſoon after, conſiſts of four hundred huts, and fifty or ſixty houſes. The former are built of timber and ſtraw, the latter of ſtones and mortar. The town, which is of a triangular ſhape, lies on the right ſide of the river Zambre, has two principal and three croſs ſtreets, and is defended on one ſide by a wall. The houſes, as well as the palace, are only one ſtory high.

At Zambre our traveller was introduced to the king, who conducted him into what he called his hall, where he diſplayed to him his treaſures. They conſiſted of a few worn copper coins, two looking-glaſſes, an almanack for the year 1743, a few

a few ſheets of printed paper; four ſmall cannons, ſuch as children have to play with; a few other toys, and a wooden clock, which ſeemed to be quite deranged. The king having, by an interpreter, expreſſed great concern that the clock did not go as formerly, our traveller, when his majeſty was gone, told the interpreter that he would try to repair it, if the king gave him permiſſion. The king aſſented; and Damberger, though not much acquainted with clock-making, was ſo fortunate as to ſucceed. When the king returned next day he was much ſurpriſed, and began to entertain a very high idea of a man who had been able to effect what exceeded the ſkill and ability of the moſt ingenious of his ſubjects. From that moment our traveller's conſequence continually increaſed; he received the ſame victuals as were uſed by the king, accompanied him every where, and, by means of this privilege, had an opportunity of getting a complete view of the lake of Zambre, when the king went thither to ſee the people employed there in fowling and fiſhing. This lake is in length three full days journey, and is interſperſed with about forty ſmall iſlands, frequented by innumerable flocks of birds. It is of an oval form, and about half a day's journey in breadth. The king keeps here a guard of two hundred men to take care that the fowling and fiſhing are properly conducted for his majeſty's advantage; but our traveller was told that theſe people dreſs for themſelves the beſt of the fowls and the fiſh that are caught.

After ſpending five months at this place, Damberger, having obtained the king's leave to depart, continued his journey on the 28th of May 1787, and on the 11th of June arrived at the firſt frontier huts of the Moohatans; a numerous and warlike nation, who inhabit a tract of land belonging to the kingdom of Monœmugi, and ſubject to its ſovereign. As the rainy ſeaſon had now commenced in this part of the country, he was obliged to remain ſome time under very unfavourable circumſtances at the village of Mytob, from which he ſet out on the 19th of July, in company with ſome travellers from the country of Moſſeguejos, lying eaſtward of the kingdom of Monœmugi, who were going to the kingdom of Otoba to fetch ſalt. After croſſing the

river Druma, and afcending a high mountain to the weft, he arrived among a people called the Mophanians, who dwell in caves, and by whom he was well received. The king of this country is an abfolute fovereign, and poffeffes a tract of territory ten days journey in length from weft to eaft, and feven in breadth from north to fouth. Of the various kinds of fruit-trees found here, the moft remarkable are the *domo* and the *inkobak*. The former bears a kind of apples without pips, of the fize of a hen's egg, having a golden colour, and a tafte like that of lemons. It grows on the mountains, has long, narrow, fharp-pointed leaves, and attains to the height of the cherry-tree. The bark refembles that of the cinnamon-tree, and is preferved along with the fruit, and ufed all the year through as a corroborant. The *inkobak* tree bears a fruit of the nut fpecies as large as an egg: it is of an oblong form, and has a red hufk, which in all probability would afford a good colour for dyeing. Our traveller obferved that the earthen-ware ufed by the inhabitants was dyed with it, and that it retained its beauty even in the fire. The nut itfelf is white, and taftes like cinnamon. The tree is as large as the oak, and all the year through bears fruit and bloffoms at the fame time.

After leaving thefe people, our author profecuted his journey through the villages of Ohgothen, Uhroh, and Mato, to Oeymoro, the refidence of the king, where he was placed among the royal flaves; but, finding means to effect his efcape acrofs the fandy deferts, he afcended the Mountains of the Moon, and arrived in the territories of the Vomahanians. Thefe people he found hofpitable and good tempered; they prefented him with tiger's flefh, which he had never before tafted, and, having directed him what courfe to proceed, wifhed him a profperous journey. He now directed his courfe northwards acrofs Dahamta to the kingdom of Volvagtam; and falling in with fome travelling negroes, who called themfelves Taomuh, he purfued his journey in their company as far as the firft huts on the frontiers of Bahura. On the 19th of November he fet out with a caravan bound to Vangara, by the way of Vadgayu, Vomy, &c.; but, not being able to bear the fatigue of riding, he was left on the road, and fell fick. On his recovery, which took

place

place ſoon after, he proceeded back again to Yandoka, and Bahahara the capital, where he was detained for a ſhort time as a ſlave; but having repaired ſome of the king's weapons he obtained his liberty, and travelled in the ſuite of the king to Kahoratho.

[To be continued.]

XII. *Obſervations on the Effects which take place from the Deſtruction of the Membrana Tympani of the Ear. By Mr.* ASTLEY COOPER. *In a Letter to* EVERARD HOME, *Eſq. F.R.S. by whom ſome Remarks are added*.*

DEAR SIR,

AT the time you were engaged in the inveſtigation of the ſtructure and uſes of the membrana tympani, you mentioned a wiſh to aſcertain the effect a rupture of that membrane would have upon hearing. I now ſend you ſome obſervations on that ſubject, which, if you think them of ſufficient importance, you will do me the honour of preſenting to the Royal Society. I am, &c.

ASTLEY COOPER.

ANATOMISTS have endeavoured to aſcertain, by experiments on quadrupeds, the loſs of power which the organ of hearing would ſuſtain by perforating the membrana tympani: dogs have been made the ſubject of theſe trials; but the reſults have been neither clear nor ſatisfactory, and they accord but little with the phænomena I am about to relate.

Mr. Cheſelden had conceived the deſign of making the human organ itſelf the ſubject of direct experiment; and a condemned criminal was pardoned, on condition of his ſubmitting to it; but a popular outcry being raiſed, it was thought proper to relinquiſh the idea.

Though denied the aid of experiment, we are not without the means of obtaining knowledge upon ſuch ſubjects; ſince the changes produced by diſeaſe frequently furniſh a clue which is equally ſatisfactory.

It often happens, that ſome parts of an organ are deſtroyed

* From the *Tranſactions of the Royal Society for* 1800.

by disease, whilst others are left in their natural state; and hence, by the powers retained by such organ, after a partial destruction, we are enabled to judge of the functions performed by those parts when the whole was in health.

Guided by this principle, I have made the human ear the subject of observation, and have endeavoured to ascertain the degree of loss it sustains in its powers by the want of the membrana tympani; a membrane which has been generally considered, from its situation in the meatus, and its connection with the adjacent parts by a beautiful and delicate structure, as essentially necessary to the sense of hearing; but which, as appears by the following observations, may be lost, with little prejudice to the functions of the organ.

Mr. P——, a medical student at St. Thomas's hospital, of the age of twenty years, applied to me, in the winter of 1797, while he was attending a course of anatomical lectures, requesting my opinion upon the nature of a complaint in his ear, which had long rendered him slightly deaf.

Upon inquiring into the nature of the symptoms which had preceded, and of those which now accompanied the disease, he informed me, that he had been subject from his infancy to pains in the head, and was attacked, at the age of ten years, with an inflammation and suppuration in the left ear, which continued discharging matter for several weeks: in the space of about twelve months after the first attack, symptoms of a similar kind took place in the right ear, from which also matter issued for a considerable time. The discharge in each instance was thin, and extremely offensive to the smell; and, in the matter, bones or pieces of bones were observable. The immediate consequence of these attacks was a total deafness, which continued for three months; the hearing then began to return, and, in about ten months from the last attack, was restored to the state in which it at present remains.

Having thus described the disease and its symptoms, he gave me the following satisfactory proof of each membrana tympani being imperfect. Having filled his mouth with air, he closed the nostrils and contracted his cheeks: the air, thus compressed, was heard to rush through the meatus auditorius

ditorius with a whiſtling noiſe, and the hair hanging from the temples became agitated by the current of air which iſſued from the ear. To determine this with greater preciſion, I called for a lighted candle, which was applied in turn to each ear, and the flame was agitated in a ſimilar manner. Struck with the novelty of theſe phænomena, I wiſhed to have many witneſſes of them, and therefore requeſted him, at the concluſion of the lecture upon the organ of hearing, to exhibit them to his fellow ſtudents; with which requeſt he was ſo obliging as to comply.

It was evident from theſe experiments, that the membrana tympani of each ear was incomplete, and that the air iſſued from the mouth, by the Euſtachian tube, through an opening in that membrane, and eſcaped by the external meatus.

To determine the degree in which the membrana tympani had been injured, I paſſed a probe into each ear, and found that the membrane on the left ſide was entirely deſtroyed; ſince the probe ſtruck againſt the petrous portion of the temporal bone, at the interior part of the tympanum, not by paſſing through a ſmall opening; for, after an attentive examination, the ſpace uſually occupied by the membrana tympani was found to be an aperture, without one trace of membrane remaining.

On the right ſide, alſo, a probe could be paſſed into the cavity of the tympanum; but here, by conducting it along the ſides of the meatus, ſome remains of the circumference of the membrane could be diſcovered, with a circular opening in its centre, about the fourth of an inch in diameter.

From ſuch a deſtruction of this membrane, partial indeed in one ear, but complete in the other, it might be expected that a total annihilation of the powers of the organ would have followed: but the deafneſs was inconſiderable. This gentleman, if his attention were exerted, was capable, when in company, of hearing whatever was ſaid in the uſual tone of converſation; and it is worthy of remark, that he could hear with the left ear better than with the right, though in the left no traces of the membrana tympani could be perceived.

When attending the anatomical lectures, alſo, he could hear,

hear, even at the most distant part of the theatre, every word that was delivered; though, to avoid the regular and constant exertion which it required, he preferred placing himself near the lecturer.

I found, however, that when a note was struck upon the piano forte, he could hear it only at two-thirds of the distance at which I could hear it myself; and he informed me, that in a voyage he had made to the East Indies, while others, when ships were hailed at sea, could catch words with accuracy, his organ of hearing received only an indistinct impression. But the most extraordinary circumstance in Mr. P——'s case is, that the ear was nicely susceptible of musical tones; *for he played well on the flute, and had frequently borne a part in a concert.* I speak this, not from his own authority only, but also from that of his father, who is an excellent judge of music, and plays well on the violin: he told me, that his son, besides playing on the flute, sung with much taste, and perfectly in tune.

The slight degree of deafness of which Mr. P. complained, was always greatly increased by his catching cold: an effect which seems to have arisen from the meatus being closed by an accumulation of the natural secretion of the ear; for it frequently happened to him, after he had been some time deaf from cold, that a large piece of hardened wax, during a fit of coughing, was forced from the ear, by the air rushing from the mouth through the Eustachian tube, and his hearing was instantly restored.

From bathing, likewise, he suffered considerable inconvenience, unless his ears were guarded against the water, by cotton being previously forced into the meatus. When this precaution was neglected, the water, as he plunged in, by rushing into the interior parts of the ears, occasioned violent pain, and brought on a deafness, which continued until the cause was removed, that is, until the water was discharged: but he had acquired the habit of removing it, by forcing air from the mouth through the ear.

In a healthy ear, when the meatus auditorius is stopped by the finger, or is otherwise closed, a noise similar to that of a distant roaring of the sea is produced: this arises from

the air in the meatus being compreſſed upon the membrana tympani. In the caſe here deſcribed, no ſuch ſenſation was produced: for, in Mr. P.'s ear, the air, meeting with no impediment, could ſuffer no compreſſion; ſince it found a paſſage, through the open membrane, to the mouth, by means of the Euſtachian tube.

Mr. P. was liable to the ſenſation commonly called the *teeth being on edge*, in the ſame degree as it exiſts in others; and it was produced by ſimilar acute ſounds, as by the filing of a ſaw, the rubbing of ſilk, &c. Its occurring in him ſeems to diſprove the idea which has been entertained of its cauſe; for it has been thought, that the cloſe connection of the nerve called the corda tympani with the membrana tympani, expoſed it to be affected by the motions of the malleus; and that, as it paſſes to nerves connected with the teeth, they would ſuffer from the vibratory ſtate of the nerve, produced by the agitations of the membrane. But, in this caſe, as the membrane was entirely deſtroyed on that ſide on which the ſenſation was produced, ſome other explanation muſt be reſorted to; and I ſee no reaſon why this effect ſhould not be referred to that part of the auditory nerve which lines the labyrinth of the ear, which, being impreſſed by acute and diſagreeable ſounds, would convey the impreſſion to the portio dura of the ſame nerve, and to the teeth with which that nerve is connected.

The external ear, though two diſtinct muſcles are inſerted into it, is capable, in its natural ſtate, of little motion: however, when an organ becomes imperfect, every agent which can be employed to increaſe its powers is called into action; and, in the caſe here deſcribed, the external ear had acquired a diſtinct motion upward and backward, which was obſervable whenever Mr. P. liſtened to any thing which he did not diſtinctly hear. This power over the muſcles was ſo great, that when deſired to raiſe the ear, or to draw it backwards, he was capable of moving it in either direction.

This caſe is not the only one of this deſcription which has come under my obſervation; for another gentleman, Mr. A., applied to me under a ſimilar complaint, (but in one ear only,) proceeding from ſuppuration, and producing the ſame effects.

effects. This gentleman has the ſame power of forcing air through the imperfect ear; ſuffers equally from bathing, if the meatus auditorius be unprotected; and feels, even from expoſure to a ſtream of cold air, very conſiderable pain. The only difference I could obſerve was, that in Mr. A.'s caſe, the defect of hearing in the diſeaſed organ was ſomewhat greater than in the former; for though, when his ſound ear was cloſed, he could hear what was ſaid in a common tone of voice, yet he could not diſtinguiſh the notes of a piano forte at the ſame diſtance: a difference which might have in part ariſen from the confuſed noiſe which is always produced by cloſing the ſound ear; or becauſe, as he heard well on one ſide, the imperfect ear had remained unemployed, and conſequently had been enfeebled by diſuſe.

From theſe obſervations it ſeems evidently to follow, that the loſs of the membrana tympani in both ears, far from producing total deafneſs, occaſions only a ſlight diminution of the powers of hearing.

Anatomiſts who have deſtroyed this membrane in dogs have aſſerted, that at firſt the effect on the ſenſe of hearing was trivial; but that, after the lapſe of a few months, a total deafneſs enſued. Baron Haller alſo has ſaid, that if the membrane of the tympanum be broken, the perſon becomes at firſt hard of hearing, and afterwards perfectly deaf. But, in theſe inſtances, the deſtruction muſt have extended further than the membrana tympani; and the labyrinth muſt have ſuffered from the removal of the ſtapes, and from the conſequent diſcharge of water contained in the cavities of the internal ear; for it has been very conſtantly obſerved, that when all the ſmall bones of the ear have been diſcharged, a total deafneſs has enſued.

It is probable, that in inſtances in which the membrana tympani is deſtroyed, the functions of this membrane have been carried on by the membranes of the feneſtra ovalis and feneſtra rotunda: for, as they are placed over the water of the labyrinth, they will, when agitated by the impreſſions of ſound, convey their vibrations to that fluid in a ſimilar manner, though in ſomewhat an inferior degree, to thoſe which are conveyed by means of the membrana tympani and the

ſmall

ſmall bones which are attached to it; and thus, in the organ of hearing, each part is admirably adapted, not only to the purpoſe for which it is deſigned, but alſo as a proviſion againſt accident or diſeaſe; ſo that, whenever any particular part is deſtroyed, another is ſubſtituted for it, and the organ, from this deprivation, ſuffers but little injury in its functions.

It ſeems that the principal uſe of the membrana tympani is, to modify the impreſſions of ſound, and to proportion them to the powers and expectation of the organ. Mr. P. had loſt this power for a conſiderable period after the deſtruction of the membrane; but, in proceſs of time, as the external ear acquired the additional motions I have deſcribed, ſounds were rendered ſtronger or weaker by them. When, therefore, he was addreſſed in a whiſper, the ear was ſeen immediately to move; but, when the tone of voice was louder, it then remained altogether motionleſs.

Some additional Remarks, on the Mode of Hearing in Caſes where the Membrana Tympani has been deſtroyed. By EVERARD HOME, *Eſq.*

After having communicated to this learned Society the very curious facts contained in Mr. Cooper's paper, which prove that the organ of hearing is capable of receiving all the different impreſſions of ſound, when the membrana tympani has been deſtroyed, it may not be improper to explain, from the obſervations contained in a former paper on this ſubject, in what manner this may take place.

It is there ſtated, that any vibrations communicated directly to the bones of the ſkull, are as accurately impreſſed upon the organ, as through the medium of the membrana tympani. The office of that membrane is therefore to afford an extended ſurface, capable of receiving impreſſions from the external air, and of communicating them to the ſmall bones of the ear; which a membrane would be incapable of doing, unleſs it had a power of varying its tenſion, to adapt it to different vibrations.

In the above caſes, in which this membrane, the malleus, and the incus, had been deſtroyed, it would appear that the ſtapes was acted upon by the air received into the cavity of

the tympanum, and communicated the impreſſions immediately to the internal organ. This not happening for ſome months after the membrane was deſtroyed, probably aroſe from the inflammation of the tympanum confining the ſtapes, and rendering its vibrations imperfect.

That ſounds can be communicated with accuracy by the bones of the ſkull to the internal organ, when received from ſolid or liquid ſubſtances, has long been well underſtood.

That the membrana tympani is incapable of perfectly anſwering this purpoſe, when ſounds are propagated through air, has been a generally received opinion; to refute which, was the object of my former paper. That, in caſes in which the membrana tympani has been deſtroyed, the air is capable of acting with ſufficient force upon the ſtapes to communicate vibrations to it, and to produce on the internal organ the neceſſary effect for perfect hearing, is completely aſcertained by Mr. Cooper's obſervations.

XIII. *Analyſis of a Stone called the Gadolinite; with an Account of ſome of the Properties of the new Earth it contains.* *By* C. Vauquelin *.

THE number of ſimple bodies, and particularly that of earths, has been greatly increaſed within a few years paſt; and if chemiſtry go on ſtill thus advancing, it is to be apprehended that the time may arrive when the human mind will not be able to embrace all the combinations produced by the multitude of ſimple bodies.

But the analogy which exiſts between certain claſſes of natural ſubſtances, gives chemiſts reaſon to ſuſpect that they contain a common generating principle, and affords them a hope that ſome fortunate chance, or an experiment made by ſome man of genius, will reduce them all to one ſingle kind, either by diſengaging them from thoſe principles which eſtabliſh differences between them, or by combining theſe ſame elements to thoſe which are ſimple.

* From the *Annales de Chimie*, No. 107.

Till

Till chemical means are brought to that degree of perfection, we muſt make known thoſe bodies which, on account of their peculiar properties, we cannot refer to any of thoſe already known, and which, till a new order be eſtabliſhed, we are obliged to conſider as new ſubſtances. An attentive examination of their properties is of great importance; for, even though they may be only modifications of bodies already known, it may happen, of which we have many inſtances, that their qualities may become uſeful to the arts and to manufactures, and, in any event, it is better to err through exceſs than deficiency.

With this view I ſhall here deſcribe, with ſome details, the properties of a new earth diſcovered by M. Gadolin, and which has been examined by M. Ekeberg under new points of view.

I ſhall firſt give a ſhort deſcription of the ſtone, and then explain the different proceſſes which I employed to analyſe it; and ſhall conclude with indicating the principal properties that characteriſe the new earth it contains. The greater part of the obſervations I ſhall make on this ſubject, may have been already publiſhed by the chemiſts above mentioned; but their works being not much known, I have thought it my duty to act in this manner, that the preſent diſcovery may be better propagated.

I ſhall, however, repeat what has been communicated to me by M. Manthey, profeſſor of chemiſtry at Copenhagen, to whom both Haüy and I are indebted for a large quantity of matter containing this new earth *.

In 1794, M. Gadolin diſcovered this earth; and his labour on the ſubject was printed in the Memoirs of the Academy of Sweden, and in Crell's Chemical Annals for the year 1796. M. Ekeberg, about two years ago, began an analyſis of the ſame ſtone, and confirmed the reſults of M. Gadolin. To the new earth he gave the name of *Yttria*, from Ytterley, a place in Sweden, where it is found. A ſhort mention of it was made alſo in the *Annales de*

* Profeſſor Abildgaard, alſo, has had the goodneſs to ſend me a pretty large quantity of this ſtone, which enabled me to vary my experiments, and to diſcover the characteriſing properties of the new earth it contains.

Chimie, No. 100. This earth, according to him, exiſts in the gadolinite in the proportion of 0·47

Accompanied by ſilex	—	0·25
Oxyd of iron —	—	0·18
Alumine —	—	0·04

He deſcribes alſo ſome of the properties poſſeſſed by this new earth when freed from all the bodies united to it in the ſtone, and which are as follows:—"All its combinations with acids have a ſweet taſte like that of the ſalts of lead, but a little more aſtringent; with the ſulphuric and acetic acids it forms cryſtalliſable ſalts which do not change in the air; with nitric acid it gives a radiating maſs, and with the muriatic acid nothing that can cryſtalliſe.

Characters of the Stone.

1ſt, This ſubſtance has a black colour, and its duſt is of a blackiſh gray.

2d, Its fracture is abſolutely vitreous, like that of glaſs.

3d, Its ſpecific gravity, aſcertained by Haüy, is 4·0497.

4th, It makes the magnetic needle move in a ſenſible manner.

5th, When expoſed to the blow-pipe, it ſplits into ſmall fragments, which fly to a conſiderable diſtance in bright red ſparks, which, when they detach themſelves, produce a ſtrong crackling noiſe. What remains of the ſtone has a grayiſh-white colour, and does not fuſe completely.

6th, When heated with borax it fuſes, and communicates to that ſalt a yellow colour inclining to violet.

7th, A hundred parts of this ſubſtance, expoſed to heat in a platina crucible, loſe eight parts of their weight, and the matter aſſumes an ochry red colour. If, from the quantity of iron it contains, we eſtimate the oxygen it muſt abſorb by the operation, we ſhall find that it has loſt about 11 *per cent.*

Phænomena which the Gadolinite exhibits with the Mineral Acids.

The gadolinite (it is by this name I ſhall diſtinguiſh this ſtone in the courſe of this memoir,) is attacked by the powerful mineral acids, ſuch as the ſulphuric, the nitric, and the muriatic;

muriatic; and, if their action be aſſiſted by a gentle heat, they form a thick jelly of a grayiſh or yellowiſh colour. If this kind of jelly be then evaporated to dryneſs, and the reſiduum of the evaporation be waſhed in water, you will obtain ſilex under the form of a white powder, which, when well waſhed and brought to a red heat, gives by its weight the proportion in which it exiſts with the other principles.

The ſolutions of gadolinite in acids do not all exhibit the ſame phænomena by evaporation. The ſulphuric and muriatic acids retain the iron and the new earth in combination, and nothing but the ſilex is ſeparated; while, on the contrary, the nitric acid abandons at the ſame time the ſilex and the oxyd of iron, which may be eaſily conceived from the properties of the nitrat of iron.

I took advantage of this property in preference to any other, in order to apply it to the analyſis in queſtion.

I diſſolved 100 parts of gadolinite in nitric acid ſufficiently diluted with water, and ſubjected it to evaporation, expoſing it to a little heat towards the end, to effect a complete decompoſition of the nitrat of iron. By re-diſſolving it in water, I obtained, combined with the nitric acid and diſſolved, the peculiar earth ſeparated from the iron and the ſilex. While my ſolution ſtill retained ſome traces of iron, which I eaſily perceived either by the reddiſh colour or by the gallic acid, I again evaporated the liquor to dryneſs, or I added a drop of ammonia, and the iron was then precipitated under the form of yellowiſh flakes, which I ſeparated by the filter.

To ſeparate the iron from the ſilex, I boiled the mixture in muriatic acid a little concentrated; I then diluted the ſolution with water, and made it paſs through the filter, in order that I might collect the ſilex, and waſh it till it was no longer precipitated by ammonia.

In regard to the new earth diſſolved in nitric acid, it will be ſufficient if it be pure to precipitate it by ammonia, and to waſh it to obtain it ſeparate; but having learned, by preliminary trials, that in this ſtate it is mixed with ſmall quantities of lime and magneſia, I was obliged to employ ſome further means for accompliſhing that end.

I never-

I nevertheleſs began to precipitate it by means of ammonia, which does not precipitate lime. I then poured into the liquor, united to the waſhings of the precipitate, ſome drops of a ſolution of common carbonat of potaſh, and I obtained the lime combined with the carbonic acid. I re-diſſolved, for the third time, in nitric acid, the earth mixed with the oxyd of manganeſe, and added, in ſmall quantities at a time, a ſolution of hydro-ſulphuret of potaſh, in order that I might precipitate only the metallic parts, which, with a little attention, I was able to effect.

I had then the earth alone, ſo that nothing was neceſſary to obtain it pure but to precipitate it by ammonia.

Analyſis of the Gadolinite by Potaſh.

I ſtill employed another method, which alſo ſucceeded, to ſeparate the different elements which conſtitute the gadolinite: it conſiſts in fuſing it with two parts of cauſtic potaſh, waſhing the maſs with boiling water, and filtering the liquor, which has a beautiful green colour.

In evaporating this liquor, the manganeſe, which gave it its colour, was gradually precipitated under the form of a black powder, which could be eaſily collected by decanting the ſupernatant water.

When it is obſerved that there is no more oxyd of manganeſe, the liquor muſt be ſaturated with nitric acid: on the other hand, the ſediment muſt be put to digeſt with the ſame acid much diluted with water: by theſe means the earth alone diſſolves, producing a great deal of heat; and the ſilex, and the iron too much oxydated, do not diſſolve.

This ſolution with the above liquor, ſaturated with nitric acid, muſt be evaporated to dryneſs, in order that if any parts of iron and ſilex have remained they may be ſeparated: in other reſpects the proceſs is the ſame as before. The latter has the advantage of ſeparating the manganeſe from the other principles, and of rendering unneceſſary an operation the ſucceſs of which is difficult.

The gadolinite might alſo, rigorouſly ſpeaking, be analyſed by directly attacking it by the ſulphuric and muriatic acids; but as theſe acids diſſolve at the ſame time, and without

without diſtinction, all the elements which compoſe this ſtone, a hydro-ſulphuret muſt be employed to ſeparate the metals; and the juſt management of this operation is difficult, becauſe the new earth is precipitated by an exceſs of the re-agent.

By the help of thoſe means, which I have briefly explained, I was able to diſcover and ſeparate the ſubſtances which enter into the compoſition of the ſtone called *gadolinite*. Theſe ſubſtances are ſilex, black oxyd of iron, lime, oxyd of manganeſe, and the peculiar earth to which M. Ekeberg has given the name of *Yttria*.

The proportions in which they are found are as follow:

1ſt, Silex — —	25·5
2d, Oxyd of iron —	25
3d, Oxyd of manganeſe —	2
4th, Lime — —	2
5th, The new earth, or *Yttria*	35
	89·5
Loſs	10·5

Theſe 10·5 form the ſmalleſt loſs I experienced in the different analyſes I made; for I thought it neceſſary to employ, in conſtructing this table, the greateſt quantities of each of the matters obtained by either of the proceſſes.

I at firſt thought that this loſs aroſe from ſome alkaline ſubſtances, as is the caſe in ſeveral kinds of ſtone; but, having treated 100 parts by ſulphuric acid, I aſſured myſelf that it aroſe from another cauſe; for, having precipitated by ammonia all the earthy and metallic matters diſſolved in the ſulphuric acid, and having brought to a red heat the ſalt produced by evaporating the liquor to dryneſs, there remained nothing in the crucible but a little ſulphat of lime.

As I ſuſpected that this was occaſioned by ſome volatile ſubſtance, I heated, in a platina crucible, 100 parts of the ſtone reduced to powder, and found that it had decreaſed eight parts, and that the remainder had aſſumed a yellowiſh colour.

The ſlight effervescence which I had always remarked

when the ſtone was diſſolved in acids, induced me to imagine that one part at leaſt of the deficit was owing to the carbonic acid.

To aſſure myſelf of it, I introduced into a ſmall phial 100 parts of the matter in powder, and, after preparing a glaſs tube, deſtined to convey the gas into the lime-water, I poured into it ſulphuric acid diluted with a certain quantity of water; a ſwelling up and heat were produced, and ſome air-bubbles paſſed into the lime-water, by which it was rendered turbid; but the quantity of the precipitate was ſo ſmall, that it was impoſſible for me to meaſure it. As the ſpace, indeed, in the veſſels which remained empty was ſo great, it is probable the greater part of the carbonic acid remained in it.

But this experiment ſufficiently proved to me that the 10·5 of loſs belonged not merely to the carbonic acid; for, though the ſpace furniſhed by my veſſels was pretty conſiderable, it would not have been capable of retaining it, and I ſhould alſo have obtained a greater quantity of calcareous precipitate. In the hope of finding ſome other ſubſtance, which, with carbonic acid, might contribute to form this loſs, I put 100 parts in a retort of luted glaſs, to which I adapted a ſmall receiver, and I expoſed it to a ſtrong heat. There appeared in the neck of the retort, and even in the bottle, ſome ſmall drops of water, the quantity of which was ſo ſmall that I could not weigh it. But the matter taken from the retort weighed no more than 91 parts. Thus it appeared to me that the loſs which I experienced in my analyſes is chiefly owing to water and carbonic acid.

Having given the moſt remarkable characters of the foſſil called *gadolinite*, and the proceſſes which appeared to me beſt adapted for ſeparating its principles, I ſhall now briefly explain ſome of the properties exhibited by the new earth extracted from it.

1ſt, It is perfectly white; but it is pretty difficult to obtain it in that ſtate on account of the oxyd of manganeſe, which follows it in almoſt all its combinations.

2d, It has neither ſavour nor ſmell.

3d, It

3d, It is not fuſible alone, but with borax it forms a white glaſs, which is tranſparent when it has not been added in exceſs.

4th, It is not ſenſibly ſoluble in cauſtic fixed alkalies: in this it is different from alumine and glucina, which combine with theſe very eaſily and in large quantity.

5th, It is ſoluble in the carbonat of ammonia, but five or ſix times leſs ſo than glucina; that is to ſay, five or ſix times more carbonat of ammonia is required to diſſolve an equal quantity of yttria.

6th, It combines rapidly with the ſulphuric acid, and produces heat in proportion as the union is effected: the ſalt thence reſulting cryſtalliſes in ſmall brilliant grains little ſoluble in water: it appeared to me that more than fifty parts of cold water were neceſſary to diſſolve it, eſpecially when not accompanied with an exceſs of acid. It has a taſte at firſt aſtringent, and afterwards ſweet, like ſugar or ſalt of lead. This property, though analogous to that of glucina, is, however, ſo ſenſibly different from it, that by comparing them they may be eaſily diſtinguiſhed.

7th, Its combination with the nitric acid has a more ſtriking ſavour, but it produces in the mouth an effect of the ſame nature: it cryſtalliſes only with difficulty, and its affinity for water is ſo great, that it requires ſome trouble to dry it. During this operation, if it be expoſed to too much heat, inſtead of becoming ſolid, like the greater part of the ſalts, it grows ſoft, and aſſumes the appearance of thick tranſparent honey; by cooling, it becomes hard and brittle like a ſtone; when expoſed to the air, it attracts the moiſture of it, and becomes ſoft.

The ſulphuric acid, poured into a ſolution of the nitrat of yttria, forms in it a cryſtalline precipitate, which is a ſulphat of the ſame earth.

8th, The combination of this earth with the muriatic acid exhibits nearly the ſame phænomena, in the ſeveral experiments I made, as the nitrat above examined: like the nitrat it can be dried with difficulty, it is fuſible by a gentle heat, and ſtrongly attracts the moiſture of the atmoſphere.

9th, Ammonia precipitates yttria earth from the three

combinations above mentioned; lime and barytes produce much more ſenſibly the ſame effect.

10th, The oxalic acid, and conſequently the oxalat of ammonia, form in its ſolutions precipitates which have an appearance abſolutely ſimilar to that of the muriat of ſilver: glucina with the oxalic acid forms a very ſoluble ſalt—a new difference between theſe two earths.

11th, Pruſſiat of potaſh cryſtalliſed and re-diſſolved in water occaſions, in the ſolutions of this earth by acids, a white granulated depoſit; but this is not the caſe in ſolutions of glucina.

12th, The phoſphoric acid does not precipitate it from the other acids, but the phoſphat of ſoda ſeparates it under the form of gelatinous white flakes.

13th, It appears to me that it has more affinity for, at leaſt, ſome of the acids, than glucina has.

14th, It precipitates an infuſion of gall-nuts in brown flakes.

From what I have ſaid, a great number of analogies may without doubt be obſerved between this earth and glucina; but, at the ſame time, there appear differences which do not permit us to confound theſe two earths. Theſe differences chiefly are, the inſolubility of the yttria and the ſolubility of glucina in fixed cauſtic alkalies; the little ſolubility of the ſulphat of the yttria, and the great ſolubility of glucina, in the ſulphuric acid; the difficult ſolubility of yttria, and the ready ſolubility of glucina, in carbonat of ammonia; the precipitation of yttria, and non-precipitation of glucina, from their ſolutions by oxalic acid and the pruſſiat of potaſh.

Here then we have nine kinds of earth very diſtinct by the properties peculiar to each: ſoon, no doubt, we ſhall reckon ten, if, as we have reaſon to preſage from the accuracy of M. Tromſdorf, the exiſtence of that which he has lately announced under the name of *auguſt*, in the Saxon beryl, be realiſed.

Theſe earths will increaſe in a wonderful manner the number of the ſaline combinations, which are already very conſiderable, and will furniſh to chemiſts a multitude of new properties to be ſtudied. It is to be wiſhed that they may find

find ſome of them applicable to the arts, in order that theſe diſcoveries may not remain altogether uſeleſs. It is to be wiſhed alſo, when the properties of this new earth ſhall be better known, that chemiſts may give themſelves the trouble to change the name of yttria, by which it is now known, and which is derived from Ytterley, the place where it is found, that another derived from its eſſential properties may be given to it.

I ſhall conclude with remarking the great difference between the reſult of M. Ekeberg's analyſis and mine. I do not know exactly to what it is owing, but I can aſſert that, in five analyſes which I made of this ſtone by different proceſſes, I never had leſs than 12 of loſs. I preſume that a certain quantity of moiſture, and perhaps carbonic acid, remained in the new earth obtained by M. Ekeberg, for it is chiefly in this point that we differ. He found 47,5, and I only from 34 to 35, becauſe I calcined it more ſtrongly.

NEW PUBLICATIONS.

A Manual of a Courſe of Chemiſtry; or, a Series of Experiments and Illuſtrations neceſſary to form a complete Courſe of that Science. By J. B. BOUILLON LAGRANGE, *Profeſſor in the Central Schools of Paris, &c.* Tranſlated from the French, with 17 Plates. 2 Vols. 18 Shillings. Cuthel, and Vernor and Hood, 1800.

[Continued from Page 282.]

IN our laſt we announced the publication of this uſeful work, and promiſed a few extracts. We now ſubjoin the following.

Citric Acid [*Concrete juice of lemons*].

Scheele was the firſt perſon who found means to obtain the citric acid cryſtallized, and well ſeparated from the mucilage, which accompanies it in the juices of thoſe fruits that furniſh it.

The proceſs for obtaining this acid, according to that chemiſt, is as follows: Expreſs the juice of lemons, and leave it

at reſt for twenty-four hours, to favour the ſeparation of the mucilage; then filter it through paper, and ſaturate it with a quantity of the carbonate of lime. The citrate of lime, which reſults from this ſaturation, being inſoluble, is precipitated to the bottom of the liquor: when this depoſit is well formed, draw off the ſupernatant liquor, and waſh the precipitate until it becomes inſipid and exceedingly white; then decompoſe this ſalt by the help of a gentle heat, with half its weight of ſulphuric acid, diluted with ſix parts of water; the ſulphuric acid takes the lime from the citric acid; the greater part of the ſulphate of lime formed is precipitated, and the citric acid remains free in the water. This acid may be obtained under a cryſtalline form, by evaporating it to the conſiſtence of clear ſyrup, and then ſuffering it to cool.

Dizé, who made ſeveral experiments on this ſubject, found that an exceſs of ſulphuric acid was neceſſary to deſtroy the portion of mucilage, which the acid obſtinately retains in its combination with the lime, and which oppoſes its cryſtallization. He obſerved alſo, that to obtain the citric acid perfectly pure, it was neceſſary to diſſolve it, and to cauſe it to cryſtallize ſeveral times.

The cryſtals obtained by Dizé were rhomboidal priſms, having their planes inclined to each other at angles of about 60 or 120 degrees, and terminated on each ſide by ſummits with four faces, which intercepted the ſolid angles.

One part of diſtilled water at the temperature of ten degrees, according to this author, diſſolves 1·25 of cryſtallized citric acid, and, during the ſolution, cold equal to 13 degrees (29° F.) is produced.

This acid reddens blue vegetable colours: when expoſed to the fire in cloſe veſſels, with a pneumatic apparatus, it is decompoſed; an acid phlegm, carbonic acid gas, and carbonated hydrogen gas, are obtained from it: and a little charcoal remains.

Its cryſtals effloreſce in the air.

An agreeable lemonade may be prepared with this acid: for this purpoſe Dizé propoſes about half a dram of the acid diſſolved in about two pounds of water, and a ſufficient quantity of ſugar and *oleoſaccharum*, made with lemon peel.

If

If a bit of ſugar be rubbed againſt a piece of orange or lemon-peel, it will imbibe the volatile oil, and form an oleo-ſaccharum, ſoluble in water, and exceedingly proper for rendering certain liquors aromatic.

[To be continued.]

Mr. BLAIR, of the Lock Hoſpital and Finſbury Diſpenſary, aſſiſted by ſeveral other reſpectable ſurgeons, has been ſome time engaged in writing a comprehenſive *Syſtem of medical and operative Surgery*, adapted to the preſent improved practice at the London Hoſpitals, &c.

Mr. WILLIAM HENRY, of Mancheſter, has in the preſs, and in conſiderable forwardneſs, a ſmall work, intended, partly, to facilitate the acquirement of chemical knowledge, to perſons entering on the ſtudy, without the benefit of an inſtructor; and, partly, as a pocket companion, for the uſe of more advanced ſtudents. The firſt part will contain directions reſpecting the beſt mode of ſtudying chemiſtry; and alſo, an arranged ſeries of experiments, neceſſary to be performed by thoſe who intend to become acquainted, by actual obſervation, with the chemical properties and habitudes of bodies. More minute directions will be given, for conducting theſe experiments with ſucceſs, than are to be found in other elementary books. The ſecond part will compriſe ſummary inſtructions reſpecting the analyſis of mineral waters, and of mineral bodies in general: and the third part will point out ſome of the uſeful applications of chemical agents, in detecting adulterations, in diſcovering poiſons, &c. The work will form one ſmall pocket volume; and it may be proper to obſerve, that it will not at all interfere with the excellent little manual, lately publiſhed by Mr. Parkinſon, the plan and objects of which are perfectly different.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ROYAL SOCIETY OF LONDON.

TO our preſent number we have prefixed a ſtriking likeneſs of the Right Hon. and learned Preſident of this Society, a gentleman whoſe unwearied zeal and princely liberality in promoting every thing that can advance ſcience or add to the comforts of mankind, place him far above any eulogy in our power to beſtow. We only ſpeak the ſentiment of the whole philoſophical world, when we expreſs our ardent wiſh that he may long continue to preſide over that learned Body, to which the world owes ſo many diſcoveries, and which has made ſuch rapid advances ſince this Right Hon. Gentleman was called to the chair he ſo ably fills.

On the 8th of January the Society met for the firſt time ſince the Chriſtmas receſs, when the concluſion of Doctor Young's paper on the mechaniſm of the eye was read.

An appendix by Dr. Herſchel, to his paper on the power of penetrating ſpace by means of teleſcopes, was read. The means extend ſo far as to diſcover nebulæ, the light of which, according to the velocity aſcribed to that fluid, muſt have been one million nine hundred and ten thouſand years coming to this earth!

A paper on impoſſible quantities, by Mr. Woodhouſe, was read the ſame evening.

Jan. 15, a letter by Mr. William Hay was read, giving an account of a ſhip ſpringing a leak in the Indian ſeas. The water, which gained ſo faſt as to render it impoſſible, by pumps and the uſual means, to prevent the ſhip from ſinking, was fortunately ſtopped by pouring rice into that part of the ſhip. The rice in four days diſſolved, and in about eight days after became ſo hard that with difficulty

it

it could be chiſeled out when the ſhip came into port. The author ſtates, that the ſame means have been tried at other times, but not with the ſame ſucceſs, and ſurmiſes that heat is neceſſary.

On the 22d was read a paper on the production of cold, by means of muriat of lime, by Mr. Walker.

The firſt conſul of France tranſmitted to the Society "Voyage autour du Monde, par C. E. Marchand," 4 tom. moſt handſomely bound. The thanks of the Society were ordered to be returned to the firſt conſul of France for this preſent.

METEOROLOGY.

The following particulars reſpecting the ſtorm on the 9th of November, furniſhed by an intelligent correſpondent, cannot but prove intereſting to ſome of our readers:

The preceding night had been ſtormy, with torrents of rain. At 9 A. M. wind abated and rain over. Thermometer 55°. Barometer, deſcending $\frac{1}{10}$ inch *per* hour, gave evident intimation of the event. About noon it had arrived at its loweſt point, having dropped from 29,9 to 28,5 ſince the morning of the ſixth. At this time it was cloudy and nearly calm, when the wind veered to the northward, and in a few minutes blew from north-weſt with aſtoniſhing violence for about an hour. The quickſilver, which had ſcarcely become ſtationary, as ſuddenly changed its direction, and continued to riſe at the rate of $\frac{2}{10}$ *per* hour the whole afternoon. It became ſtationary at 29,7, with a hoar-froſt in the morning. The index-hand of an excellent wheel barometer, fixed againſt a ſolid wall, was obſerved to librate continually during this rapid aſcent, retreating about $\frac{1}{100}$, and then advancing a little more. In the ſpace of about ſix hours, on this occaſion, the atmoſphere paſſed through near one-half of its total variation of gravity, which would require, in its ordinary courſe in this country, about as many days.

This impetuous irruption of denſe air from the northern regions was probably occaſioned by the ſudden rarefaction conſequent upon a general precipitation of that water which during

during the preceding four weeks had been accumulating in the atmosphere in a dissolved state. The loss, in a certain space of atmosphere, of so great a quantity of elastic fluid, and the operation of its disengaged caloric on the remainder, will at once account for the effect to those who are acquainted with pneumatics.

THE NEW ALKALI.

In our last (p. 291) we mentioned that a Mr. Hahneman, of Altona, had announced the discovery of a new alkali, to which he gave the name of *pneum*, from its property of swelling by heat to twenty times its original volume. Both Klaproth and Hermbstadt have lately examined this alkali, and found it to consist of nothing but *borax*.

ALKALINE PRUSSIAT.

Dr. A. N. Scherer has (in his journal) recommended to chemists to take up the idea suggested by Scheele, and to endeavour, by following the hint given by that author, to produce a perfectly pure alkaline prussiat. Scheele states that prussiat of mercury is perfectly free from iron, and that, were a combination of that kind decomposed by an alkali, the result would certainly be prussiat of alkali entirely free from iron.

A NEW EARTH.

The particular properties of a new species of earth called *Yttria*, of which an account was given by Ekeberg in the third volume of Scherer's journal, have been confirmed by Klaproth, who lately read a paper on this subject at the Royal Academy of Berlin. Vauquelin has also analysed the stone which produces this earth.—See his Memoir on this substance in the present number, p. 366.

CHEMICAL EXPERIMENTS ON THE MATTER OF BLACK VOMIT.

A memoir on the analysis of black vomit, by Dr. Cathrall, was read before the American Philosophical Society at Philadelphia, on June 20, 1800. This is a very interesting and instructive paper. The experienced and intrepid author has given a description of the black vomit, has analysed the fluids ejected a few hours before the commencement of black vo-

miting, and exhibited a ſet of experiments on the matter of black vomit itſelf: to which he has added, experiments to aſcertain the effects of black vomit on the living ſyſtem of man and other animals, and a ſynopſis of the opinions of authors concerning its formation and qualities. The experiments ſhow that this ſingular morbid excretion contains an acid, which is neither carbonic, phoſphoric, nor ſulphuric; and, what our readers will hardly expect, that the black vomit may be ſmelled, taſted, and ſwallowed, without inducing yellow fever, or even any ſickneſs at all: ſo little infection or contagion does it ſeem to contain! He concludes it to be an altered ſecretion from the liver.

TETANUS CURED BY THE COLD BATH:

Communicated in a Letter to Dr. R. H. ARCHER, *of Baltimore, by Dr.* WILLIAM HARRIS, *of Pennſylvania*.*

In the autumn of 1799, I viſited a labourer, about thirty years of age, of a ſlender make, but healthy, who was ſuddenly ſeized, whilſt in bed, with ſpaſms in his lower extremities, which ſhortly after affected his whole ſyſtem, but particularly his ſtomach, which was drawn in a hard lump, and protruded to a conſiderable diſtance. His pains were excruciating. He had a violent vomiting and purging, which came on an hour after ſeizure, and continued about two hours. At one time he had emproſthotonos, at another opiſthotonos, to the greateſt degree, and ſometimes complete tetanus. The muſcles of his face were drawn in every direction, and deglutition entirely impeded. His pulſe varied much, but was generally feeble. He could aſſign no cauſe for the attack. I bled him, put him in the warm bath, and uſed all the remedies laid down by medical writers, but without any mitigation of his pains, or relief to his ſpaſms.

At this time, which was twenty hours after the attack, when the cold ſweat of death appeared to be upon him, his tongue had refuſed its office, his eyes ſunk, having a glaſſy appearance, and his exit was every moment expected, it occurred to me that the cold bath might have a good effect; and, after conſulting his friends, who readily acquieſced, I

* *Medical Repoſitory*, vol. iv. p. 76.

had him, in this ſtate, carried in a blanket to a forge-dam which was at hand, and plunged in. He was then inſenſible. His ſpaſms immediately abated, and, in twenty minutes, totally ceaſed. The debility induced by muſcular exertion was ſuch that it required ſeveral days before he could be removed; after which he rapidly recovered, and is at this time perfectly well.

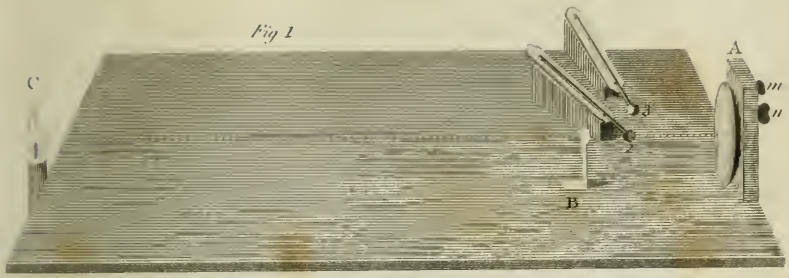
This was a complete tetanus; and, I think, tetanus from wounds, &c. would yield to the ſame mode of treatment.

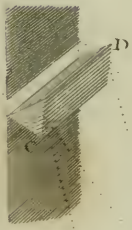
TREATMENT OF LOCKED-JAW BY ELECTRICITY.

Dr. Samuel Perry, of New-Bedford [*America*], has communicated to the public the ſucceſsful reſult of two experiments, in curing the locked-jaw by means of electricity. Previous to the application of the electrical fluid, recourſe had been had to bleeding, cathartics, antiſpaſmodics, the warm bath, and opium applied internally and externally, without the leaſt effect in either caſe. But a ſmall receiver being filled, and diſcharged through the jaws of the perſons affected, they flew open inſtantaneouſly. In one caſe the complaint was entirely removed by three ſhocks; in the other, by an occaſional ſhock for a few days. Both the patients were ſtrong and healthy perſons, the one a man and the other a woman, and the mode of treating them had been ſimilar.

Dr. PEARSON's Lectures on the Materia Medica, Practice of Phyſic and Chemiſtry, will recommence at the Elaboratory, Whitcomb-ſtreet, Leiceſter-ſquare, on Tueſday, February 3, at eight in the morning.

Fig 1





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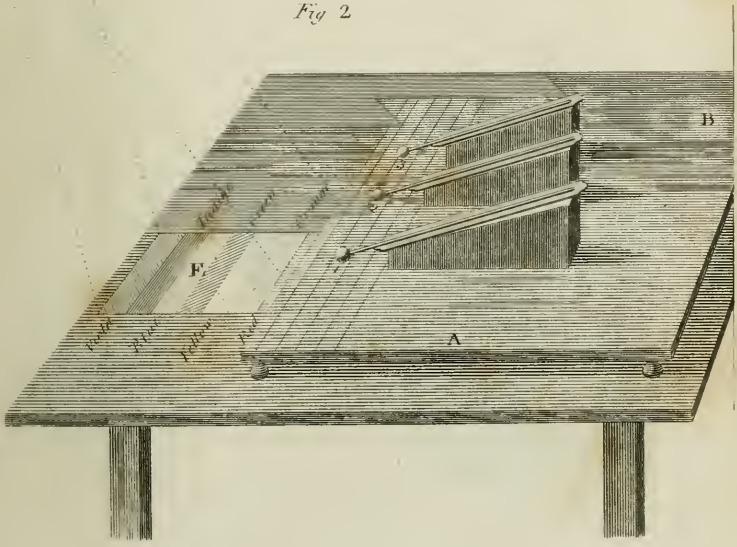

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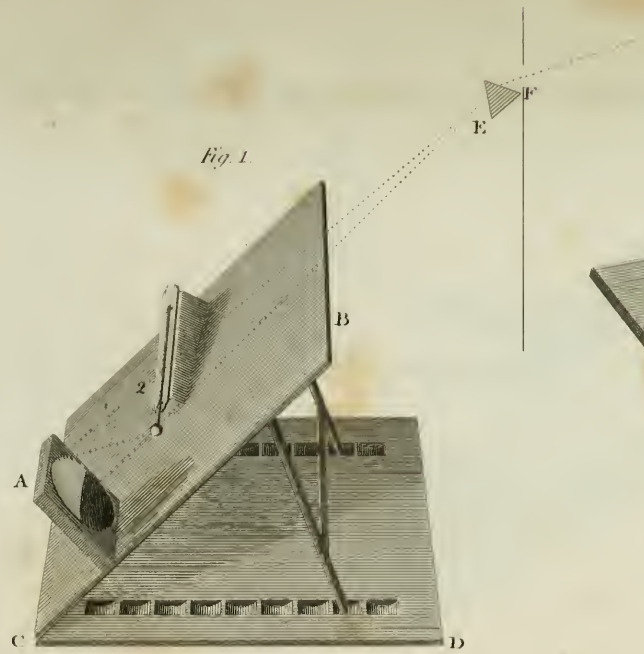

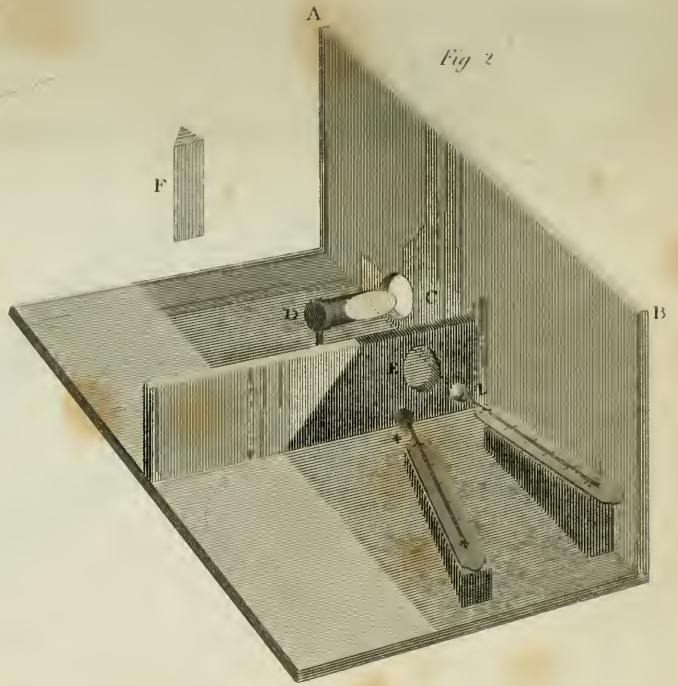

Lowry, sculp.

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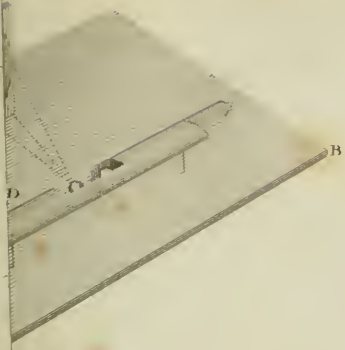

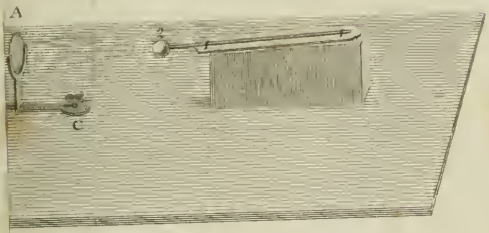

Fig. 3.

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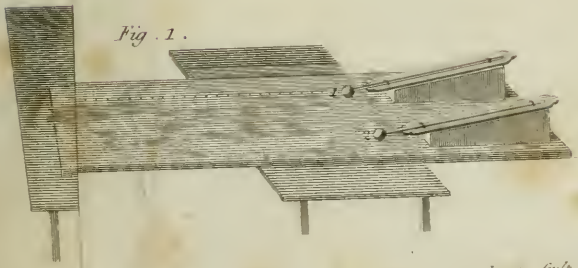
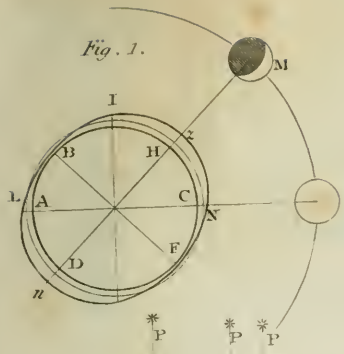


Lowry sculp.

Fig. 1.



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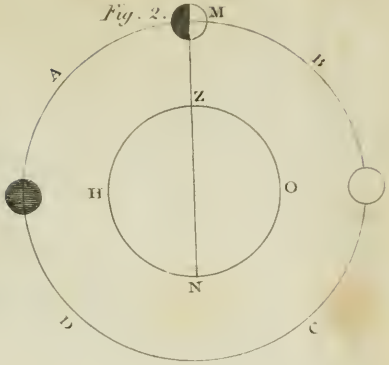

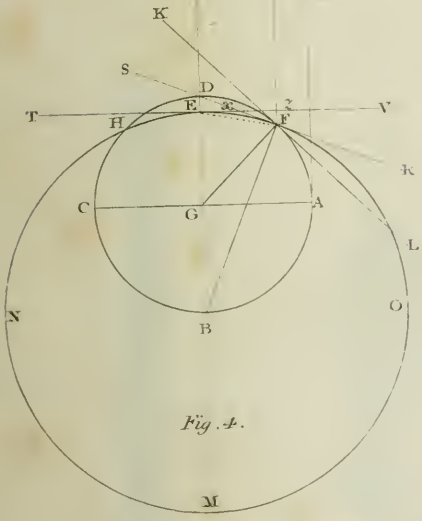

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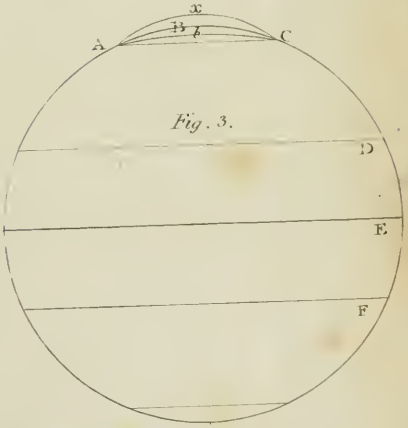
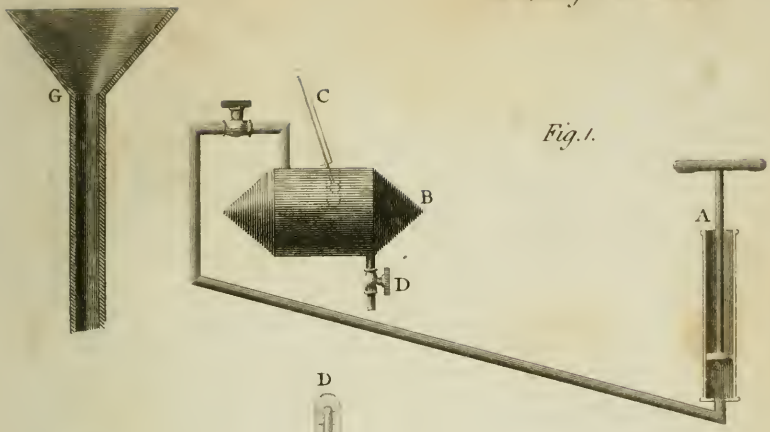


Fig. 3.

Lowry sculp.



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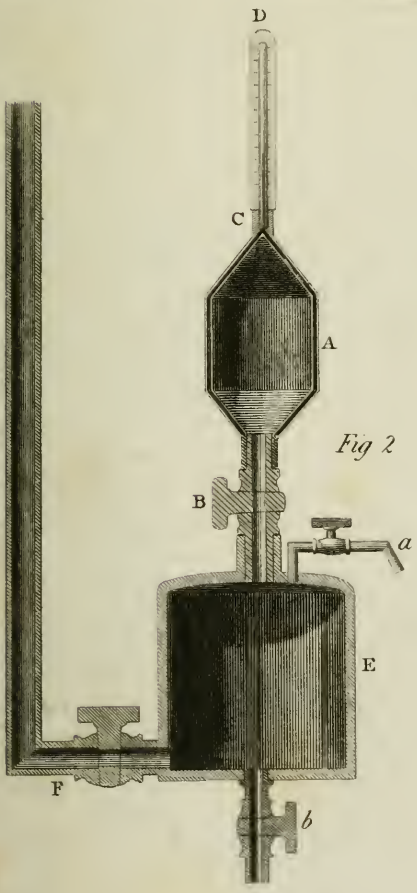

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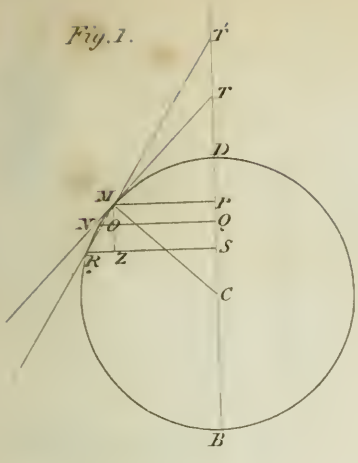

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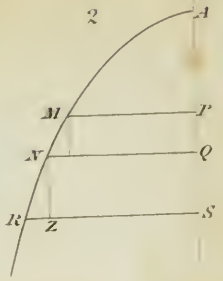

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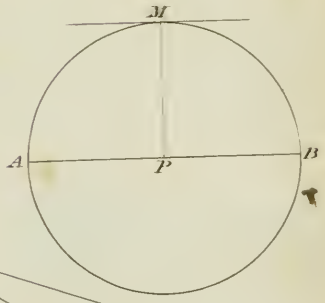

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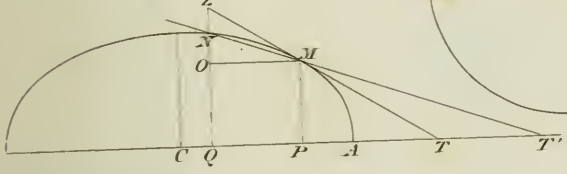

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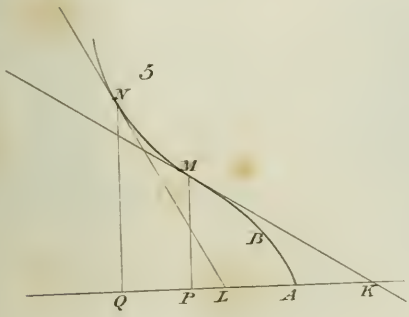

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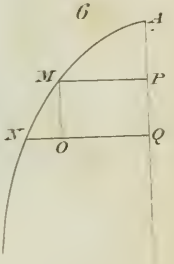

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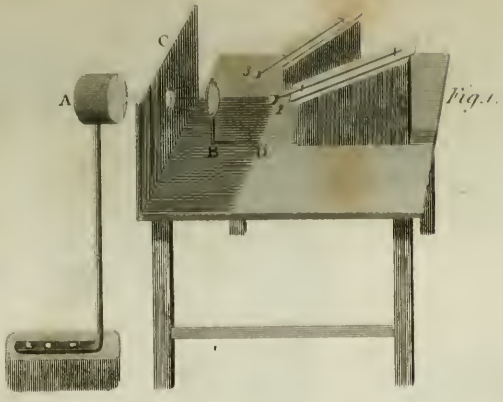

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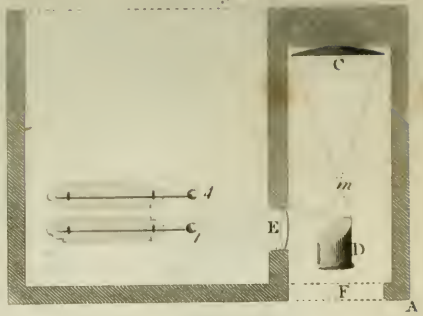

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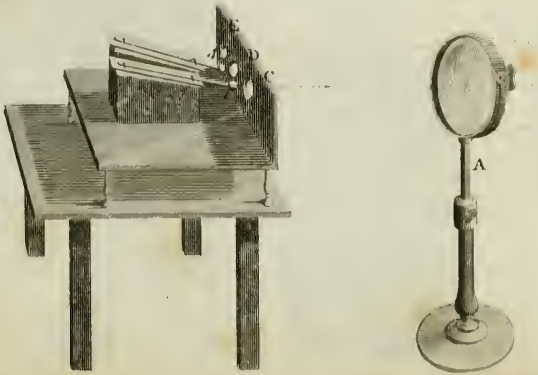

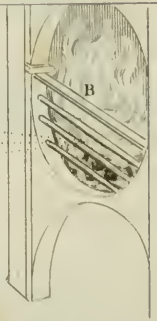

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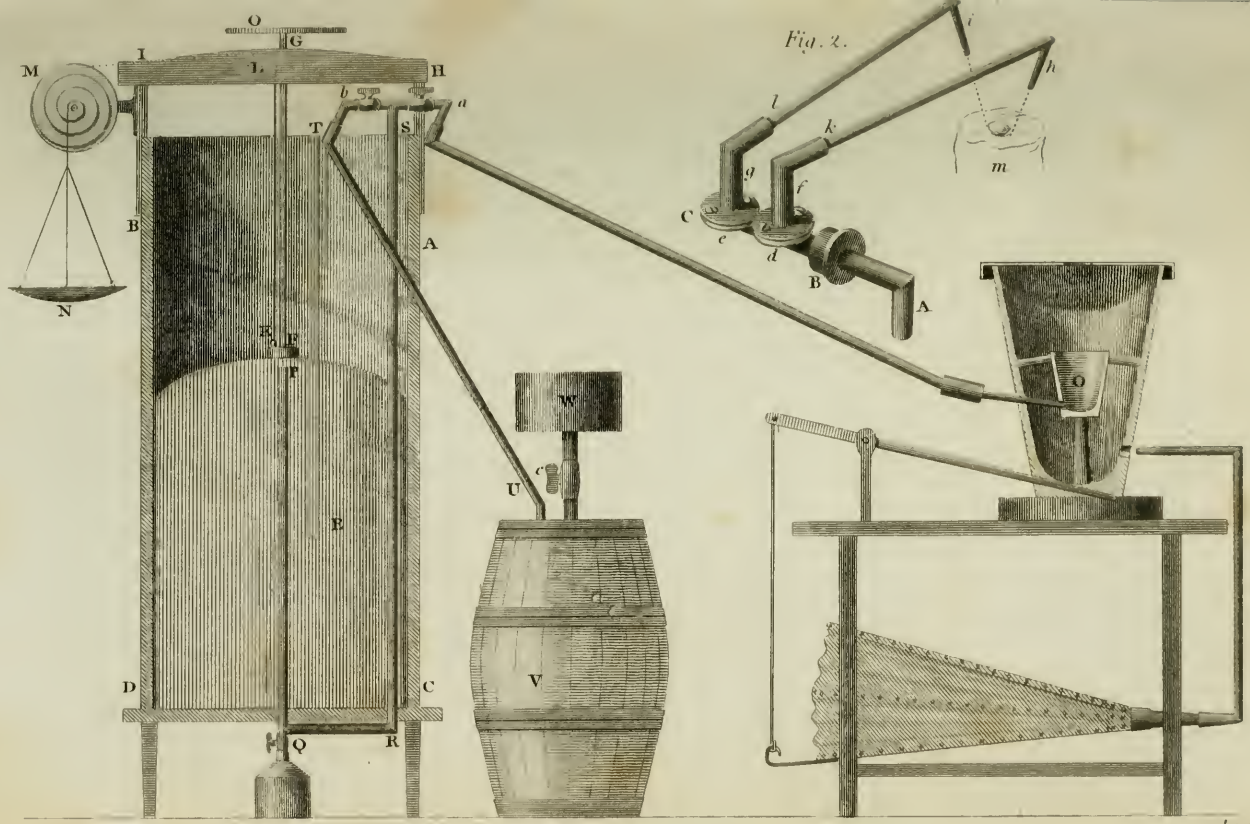
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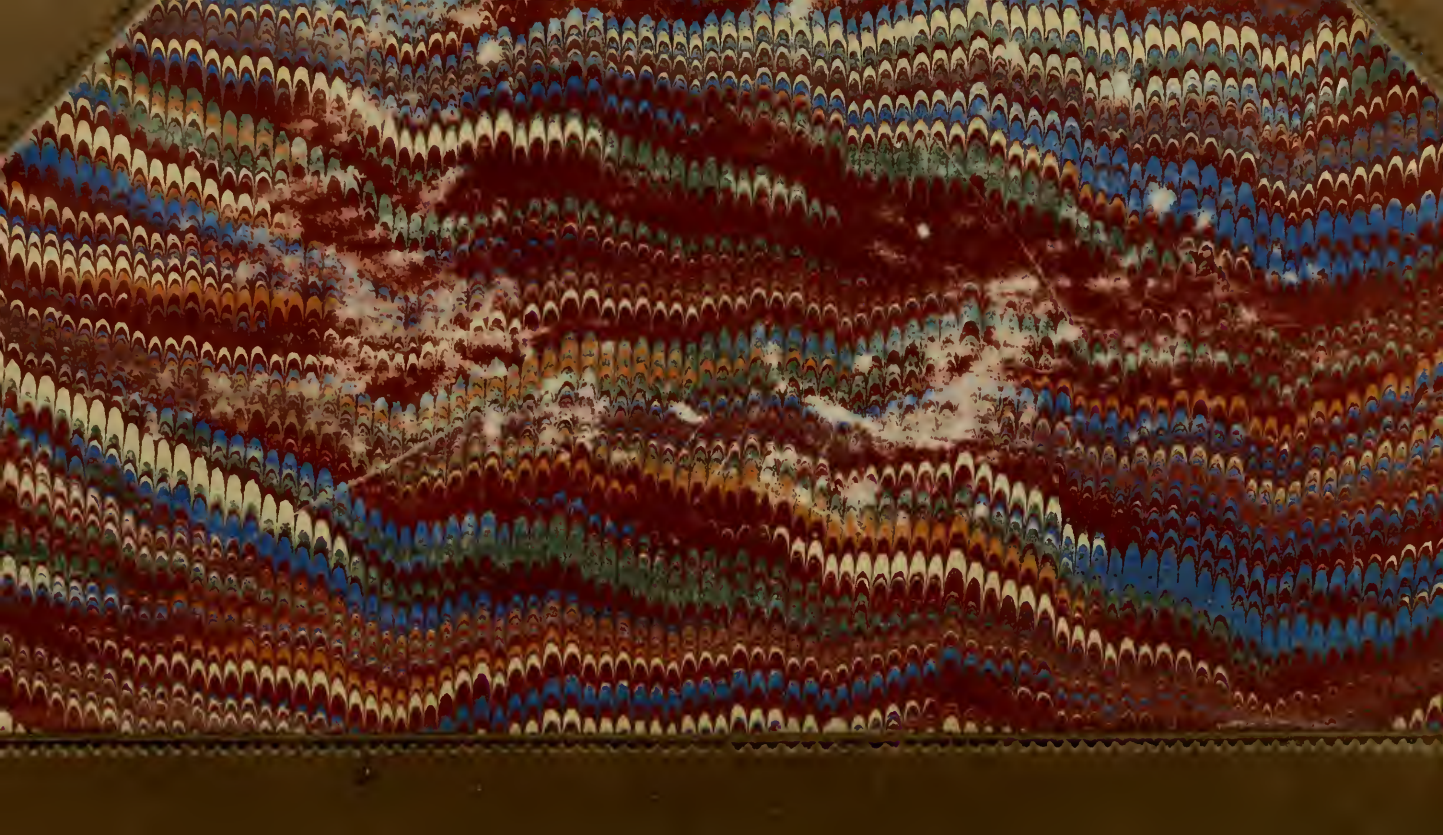
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